THE POPULAR JOURNAL OF KNOWLEDGE

DISCOVERY

CAMBRIDGE: SEPTEMBER 1938

- Forty Years of Evolution Theory. By R. C. PUNNETT
- Maxwell's Demon. By L. INFELD
- Water Divining
- Notes on Nutrition
- Second Inquest on Detective Stories

The British Association Meeting at Cambridge

NE
HILLING
et

DISCOVERY

New Series, Vol. I, No. 6 SEPTEMBER 1938 **1*s*. net**

Published by the CAMBRIDGE UNIVERSITY PRESS, Bentley House, 200 Euston Road, N.W. 1 (Tel.: Euston 5451–3)

Editor: C. P. SNOW, University Press, Cambridge (Tel.: 4226)

Advertisement Offices: 61 Chandos Street, W.C. 2 (Tel.: Temple Bar 6897)

Annual Subscription, 12*s*. 6*d*. net post free

Science and Dowsers *

PROFESSOR C. D. BROAD, when asked what his reactions would be to a definite proof of human survival after death, is said to have replied, "I should be more annoyed than surprised." If a scientist were to obtain evidence that dowsers can find hidden things by means other than the use of their five senses he would question that evidence, for it would amount to presenting physiologists with a new sense and might be expected to involve principles unknown to contemporary physical science. If reconsideration and further tests confirmed the original conclusion he would probably be more surprised than annoyed, for he would be opening a gateway to new discoveries.

There can be no doubt, however, that all the *reliable experimental* evidence shows that dowsers cannot find hidden things by means other than the use of their five senses. Ten typical cases are as follows:

(1) Major C. A. Pogson of Hove claimed to be able to tell which pole of a battery was positive, which pole of a magnet North-seeking; to distinguish open jars containing fresh water from those containing either paraffin or salt water. Tested with respect to all of these claims he always got definite responses, but the agreement of the latter with actual facts was consistent with the hypothesis that it was due to chance expectations.

(2) Mrs Pogson of Hove, in spite of obtaining correct responses when she knew the answer and definite responses when she did not, did no better or worse than

* A review of *Water Divining: New Facts and Theories*, by THEODORE BESTERMAN (Methuen).

chance expectation. She attempted to distinguish the electrical polarity of metal plates, to find water or radium in open jars from similar jars left empty, to distinguish pennies from half-crowns and copper from platinum. In addition she failed to name correctly the metals in a given purse and to give correctly the depth of water in the reviewer's garden.

(3) Miss Penrose of British Columbia, Canada, was found by C. V. C. Hubert of the Society for Psychical Research to give marked responses when she thought that a small wireless generator was on. Her responses did not correspond to the actual state of the generator.

(4) The present Bishop of Bath and Wells was able twelve times running to get different reactions to copper and silver in envelopes when he knew the answer. After shuffling up the envelopes the degree of success of his responses did not differ significantly from chance expectation.

(5) J. Timms of Oxford had considerable success in finding hidden objects and even in distinguishing gold from lead as long as people in the room knew which of the closed pill boxes contained the gold. According to Prof. T. R. Merton, Timms failed completely when no one in the room knew which box contained the gold.

(6) Captain W. H. Trinder of Brockenhurst was able to get a response to stationary water in a tumbler when he knew it was there. When he no longer knew the answer he did no better than chance expectation.

(7) A professional dowser from Benson, near Oxford, got no response to water in a bucket, to a stream when he stood on a bridge above it, or to water flowing in a pipe underground. In all cases he said that the water was "dead water", and claimed that he could find natural underground water.

(8) H. M. Budgett of Kirtlington Park has been tested by E. C. Bullard in Cambridge and was unable to find water even if he were driven over streams in a closed van. He also failed to find a neutron source.

(9) A doctor in Mentone who specializes in "radiesthesia" at first offered to do tests and then admitted that he could not even find 2 grams of radium, and that 200 of the best "radioesthesistes" in France had failed at this test in front of Madame Curie. He claimed to diagnose illness by pendulums and had a cabinet full of letters from patients saying he had saved their lives. He was not directly tested for diagnoses of diseases, but "faith healing" seemed a possible alternative explanation of his success as a doctor.

(10) A London surgeon was alleged to be able to divine (by a London doctor in conversation with a patient). A test was arranged but the surgeon did not turn up.

Several other dowsers have been tested by the reviewer, who has a standing offer of 200 guineas to any dowser who will equal or better a 10,000 to 1 chance in front of him. People have tried continuously since 1933 and failed in all cases. Many scientists have tested diviners, but none of them has published results that convince the world of science that dowsers really can find hidden things by other means than their five senses.

It may therefore be concluded that *dowsers cannot divine under laboratory conditions and against known odds*. The above conclusion does not finally settle scientific problems arising from the general belief in dowsers. Superstitions and genuine phenomena can both hide in regions where direct experimentation is impossible, is prohibitively difficult, or prohibitively expensive. The last reason prevents a complete analysis by very numerous borings of the successes and failures of dowsers at practical problems in the field to decide how much is due to chance, to experience, to a knowledge of details of other wells in the neighbourhood, and to a knowledge of geology. An investigation however of the psychology of dowsers and of people who may employ dowsers might well be a profitable subject of study. Do people wish to believe in dowsing, and if so, why? Do employers of dowsers admit and publish the failures of dowsers to the same extent as they publish and admit their successes? What is the attitude of dowsers to their own mistakes and to those of other dowsers? What is the folk lore of dowsing and its psychological and historical origin? What are factors making for a high survival value in the case of superstition? The actual subject of the folk lore of dowsing is discussed by Theodore Besterman in *Water Divining: New Facts and Theories*, and it is to be hoped that he will continue these studies and eventually present the public with another book on the psychology of the subject.

The rest of his book deals with the evidence of results obtained in the field by famous dowsers. The best performances seem to have been put up by Major C. A. Pogson who failed in the laboratory tests at Cambridge, and by Mrs E. C. B. Dale who was ill and therefore unable to do laboratory tests for the reviewer. The book also deals with Mrs Pogson, Captain W. H. Trinder, and mentions J. Timms but does not discuss Miss Penrose who is stated to have found many mines for the Government of British Columbia.

Mr Besterman puts forward quite a good case for believing that water diviners have genuine powers as regards tests in the field, but the results of laboratory tests make the reviewer more critical than perhaps he would otherwise have been. Major Pogson was employed by the Government of India both to find water and also to test out and to train Indian diviners. He told the reviewer that he had found no Indian good enough even to be his assistant. He carried out surveys in thirteen districts. He prospected 124 villages for drinking water, and correctly located 104 sites; he also prospected 750 fields for water either for agricultural or for drinking purposes and he located correctly 465 sites. His failures, as opposed to statements that he could find no water, are stated to amount to only $1\frac{1}{2}\%$. The fields varied in size from $\frac{1}{4}$ to 50 acres, the diameter of a well when sunk was 50 ft., the depth did not exceed 50 ft., and they were mostly in alluvial deposits in which there occurred what Major Pogson described as "sort of Charing Crosses of intersecting streams of water". It is difficult to judge the true significance of Major Pogson's results in India. After returning to England he studied geology and Besterman gives records of sixteen successes of Major Pogson in finding water.

It is not stated whether these sixteen are selected cases or the only cases where Major Pogson has attempted to find water. It appears that Major Pogson cannot predict the quantity of water available (rate of recuperation) to any degree of accuracy, but the mean depth of the source of the water from ten measurements in the first nine cases was 66 ft. with a standard deviation per estimate of 7·3 ft. for the difference between the expected and the actual depths. The estimates exceeded, as any diviner would wish them to do, the actual depth by a mean value of 6 ft. Some of the cases recorded seem to be fair finds. Others refer to well-known water-bearing strata such as the three layers of Northampton Marlstone (case 11), Lincoln limestone (case 12) and Sussex chalk (cases 4, 8, 9). In other cases he merely recommended increasing the depth of existing wells (case 6).

Enough has been said to show that the performances of Major Pogson do not compel the belief that his ability to find water in the field in England is better than should be expected of a competent geologist. In the absence of further data the same is true for his performances in India. In spite of this it cannot be denied that Major Pogson still appears to have a claim to being skilled at finding water in the field; so skilled, indeed, that although Besterman cannot be said to have proved his case, neither can the reviewer be said to have destroyed completely Major Pogson's claims to success in practical problems. The final verdict about dowsing in the field has still to come. In the mean time no student of the subject can afford to miss Besterman's book.*

OLIVER GATTY.

* [The reviewer has recently obtained information about the performance of Major Pogson in the field in India. It appears that in the Kurram Valley, Parachinar, on the Afghan border, many wells were sunk on the advice of Major Pogson, but no water was found. It seems therefore that Major Pogson's performances in the field were not everywhere of the high level claimed by Mr Besterman, and indeed it seems that further investigation would show that his performance out-of-doors was little better than that recently found in the laboratory.]

Dr F. Fraser Darling gives an intimate account of Shags at home.

"We are not Amused"

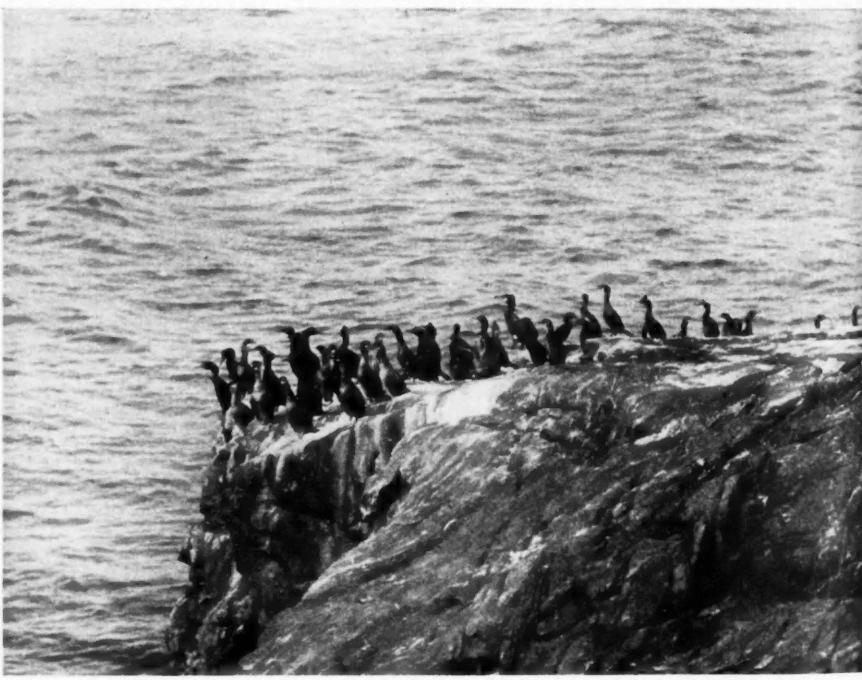
THERE seems to me no better title for this picture than this famous remark. These two young shags, which are not quite ready to leave the nesting ledge and object strongly to my interference, make a burlesque of a certain type of humanity—the kind which says, "I never heard of such treatment; I shall complain to the management!" Funny as the picture may be, it is not one of which a bird photographer can be proud, for here are two very frightened little birds who feel there is no escape from the few square feet of rock where they have been reared, sixty feet above the sea, and there is nothing funny in that. The fright was momentary, however, and I have no regrets.

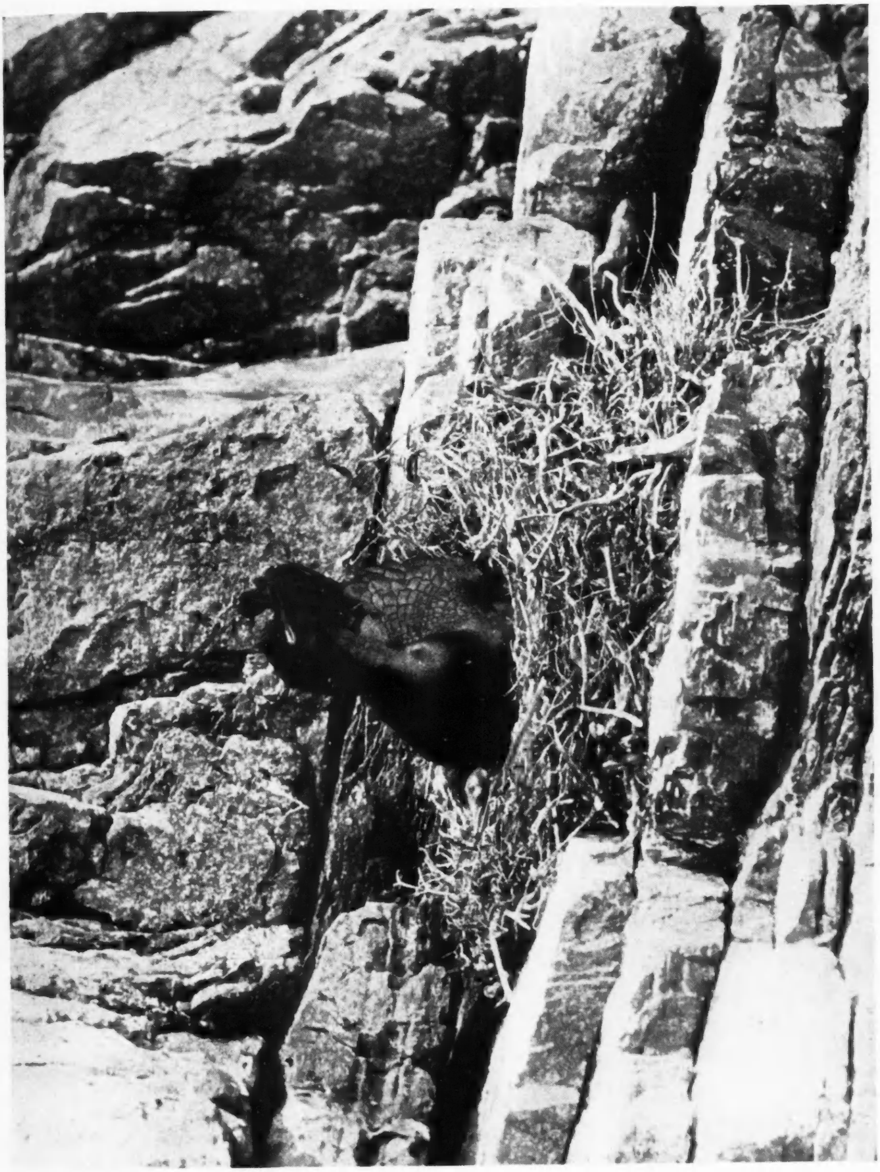
One or two points might be noted before we leave them. The shag's bigger relative, the cormorant, has a large patch of bare skin under the lower jaw, but the adult shag is feathered all the way. Presumably the ancestral type of the pelican tribe had a bare throat and the young shag exhibits this character though

it is lost by the time the bird is ready to take to the sea. Look at the large expanse of foot of four webbed claws, with the longest on the outside, an arrangement which makes steering very sensitive. These propel the bird under water and the legs are flexed or extended both at once, not alternately like a duck's when swimming, and the speed of the shag's movement is considerable.

Note also the flat tail of many evenly-ribbed feathers which is used not as a rudder to affect direction under water but for elevation. If you watch a shag diving, it jumps almost off the water and goes down head first and the tail is turned downwards to help the descent through the water. Raising the tail allows rapid ascent to the surface once more. The wings are organs of flight only and are not used under water as are the sharp narrow wings of the auk family.

Shags remind me of pterodactyls, and they become familiar with the water long before they can fly. It is not possible for these birds to learn to fly from their nesting ledge, and when fledged they just flop clumsily down into the sea. But once in the water they dive and swim like adepts and the drops run smoothly off their oily plumage. They learn to fly by walking out of the water on to shelving skerries, from the top of which they can make short flights into the sea. Such rocks become traditional gathering places for shags and cormorants, and in the picture below you see the steep side of what I call a "social" rock in early summer.





A mother shag on her nest. Her plumage is a brilliant black-green and it is shining in the sunlight

A hundred or more of both species stand there for several hours in the sunshine. There is no quarrelling or fuss. The birds preen themselves and extend their wings to dry in the sun, as if their feathers were washing on the line. Quite suddenly one decides it is time to go fishing and he drops off the rock to fly at a fast, steady pace just above the surface of the sea. The others follow in twos and threes until the whole group is strung in a black line across a mile of ocean. Only a great black-backed gull is left, picking over a few fish bones thrown up by the shags and cormorants.

Had we startled these birds instead of watching them leave of their own accord, we should have seen the wary black-backs fly off first, then the cormorants and lastly the shags, and the latter two species would not have flown many yards before dropping to the water and diving. This rock becomes a stinking place in hot calm weather, but it is never long before the wind comes and a storm from the north brings great seas pounding over the shags' "club". Next morning the rock is bright and clean and the birds return to it happily.

A social rock at the beginning of July after a dry calm period. Two days later heavy rain washed the rock clean

Forty Years of Evolution Theory

By R. C. Punnett

(Arthur Balfour Professor of Genetics, University of Cambridge)

In the minds of almost all who are familiar with his name Charles Darwin stands for the concept of Evolution, for the essential unity pervading the diversity of organic form. Hence in assigning a time limit to the "Darwinian era" we may clearly take as our starting-point 1859, the year which saw the publication of *The Origin of Species*. So long as men believe in evolution, so long in that sense will the Darwinian era continue to be with us. But since for our present purpose some circumscription of its boundaries is necessary I will take as the latter limit the year 1900, the year in which the rediscovery of Mendel's work began to lead to that reorientation of the biological sciences which is still actively going forward. And the question that we have to ask ourselves is—what were the peculiar characteristics of this period of forty years, both in relation to the time that went before and the time that follows after? What were the main ideas prevalent before, during and after the period we are setting out to consider?

Any conception of a process of evolution naturally challenges the problem of causation. These matters were long ago discussed by the ancient Greeks. Into their views I do not propose to enter, but will merely refer those interested to Osborn's *From the Greeks to Darwin*, where he sums up as follows the results of his delvings in that ancient mine:

"The Greeks left the later world face to face with the problem of Causation in three forms: first, whether Intelligent Design is constantly operating in Nature; second, whether Nature is under the operation of natural causes originally implanted by Intelligent Design; and third, whether Nature is under the operation of natural causes due from the beginning to the laws of chance, and containing no evidences of design, even in their origin."

Nor for many centuries did any choice among these three views of causation materially trouble mankind. For all their many virtues the Romans appear to have been singularly lacking in curiosity. Though the collecting instinct was by no means lacking among them, it seems to have been directed almost exclusively to the acquisition of man-made things such as books, pictures, sculptures and religions. In spite of opportunity there seems to be no evidence of the getting together of anything resembling a museum of natural history objects. Even Pliny, most curious of the Romans, contented himself with the filling of notebooks. And the time when curiosity might have led to collection, and this in turn have stimulated classification and inquiry into causes, rapidly passed away; for with the establishing of Christianity a new mental atmosphere possessed the world. The problem of causation was definitely determined in favour of the first of our three alternatives, and men's thoughts were turned to that future life which had been so opportunely called into existence to redress the balance of the present. It is true that a few of the Church's authorities—notably Augustine and, later, Thomas Aquinas—flirted at times with the second alternative, but in this they had little influence on the great body of the Church and its supporters. Routine marked out the path of the shepherds and the flock ovinely followed. Then came the crash of the Renaissance. Curiosity, so long pent up,

surged forth, and natural objects once more became objects of natural inquiry. Contributory were the voyages of exploration now starting, bringing with them new and strange forms of plant and animal life. Collections of natural history objects were brought together, and as they grew in magnitude some system of classification became imperative. In this field the botanists were in advance of the zoologists. Partly this lay in the importance which medical practice attached to plants. The early herbals were compiled from a medical standpoint, but as one succeeded another it became apparent that many plants have points of resemblance to one another entirely unconnected with either their medicinal properties or their importance to agriculture.

* * * *

There gradually grew up the feeling that plants can be arranged in natural groups, and this ultimately led to the production of works in which more and more stress was laid on a natural system, and less and less upon medicinal properties. The animal kingdom, with its vastly greater varieties of form, offered more difficulties. The earliest compendium, Conrad Gesner's *Historia Animalium*, is a queer chaos for the modern reader. Bats are classed among birds, and whales among fishes. Gradually a more and more natural grouping evolved, and by the middle of the eighteenth century was established the famous *Systema Naturae*, a classified conspectus in which Linnaeus arranged animals and plants according to Classes, Orders, Genera and Species. Its acceptance by the scientific world was a formal recognition of a *natural* order in the world of living things. Since the time of Linnaeus classification has undergone enormous development, but without in any way upsetting the conception of such a natural order. When two systematists disagree we should say that both may be wrong, but that only one can be right. Belief in the conception naturally led to speculation as to what lay behind it. Linnaeus and most of those of his time believed that the natural order was the manifestation of the mind of God. The species was as it had issued from the mind of the Creator, though it was conceded that in this evil world slight modifications might at times occur through hybridism or other naughtiness. In other words the species was a constant and immutable thing.

It not infrequently happens that a debatable doctrine may be readily accepted owing to apparently strong support from another which subsequently turns out to be false. Such was the case with the doctrine of the fixity of species. During the whole of the eighteenth century biological opinion was almost entirely under the sway of the curious and erroneous Theory of Preformation. According to it every living thing was brought into existence at the moment of creation, all future generations being preformed and packed away in the germ-cells (for some the ova and for others the spermatozoa) of the original progenitor. Thus for the Ovists the whole of the human race was contained in Eve's ovary, and successive generations were merely an unfolding, or *evolutio*, of the original creation. The doctrine of the fixity of species was a natural corollary of the doctrine of Preformation, and both were heartily endorsed by the Church. For thus the supreme act of creation stood revealed in its true proportions, while at the same time were avoided both troublesome thinking and dangerous speculation.

* * * *

Most biologists agreed with Linnaeus in accepting the natural system as divinely appointed. Efforts, such as those of Bonnet, to show that living things could be arranged on a progressive scale, with man of course at the head, were made with due reverence to the Almighty, and without thought of any genetic continuity. It is true that here and there voices were raised in an opposite sense, but none was commanding enough to attract a following of any weight. Buffon,

popular as he was, vacillated overmuch, nor was his reputation too good among those who passed for men of science: Erasmus Darwin, though a vigorous and original thinker, was too insular, nor was his vehicle of flowery verse well suited for the purpose: others, such as de Maillet, Robinet and Diderot, were never effective because their speculations, bold as they often were, lacked a sound knowledge of structure on which to base them. For all this while, comparative anatomy was making active progress, culminating in the achievements of Cuvier and his school. Cuvier himself, the founder of palaeontology, was a firm adherent of the theory of special creation, and his influence was immense. Had it not been for him it is possible that Lamarck's views would have attracted more support, and that contemporary copies of the *Philosophie Zoologique* would not be among the rarities of zoological literature.

From Lamarck to Darwin, from the *Philosophie Zoologique* to *The Origin of Species*, is exactly half a century. It may perhaps be termed the Cuvierian era, for it was largely dominated by the concepts of the great French anatomist. For the zoologists it was essentially an age of description and classification, far fuller and more accurate than any hitherto accomplished. Characteristic of it is the production of the great *Règne Animale*, in which neither classification nor sentiment in any way clashed with the basal concept of special creation and the fixity of species.

* * * *

Nevertheless, there were signs that all were not satisfied with the dominant outlook. It was not enough to record living things, however accurately and sumptuously, merely as manifestations of the power and wisdom of the Creator. Might there not be some purpose behind it all? The idea was in the thought of Kant. Organisms are composed of parts which are only comprehensible as conditions for the existence of the whole. The very existence of the whole thus implies an end. Though nature exhibits nothing to us in the way of purpose we can only understand an organism if we regard it *as though* produced under the guidance of thought for an end. Here we are very close to the innate "Perfecting Principle" of Aristotle. Kant's thought influenced Goethe. He regarded a living being as a complex of different elements each referable to a primordial type. Thus, the various parts of the flower are referable to a primordial leaf: thus too are the various segments of the back-boned animals referable to a primordial vertebra. The primordial type becomes modified in this direction or in that owing to its different position in the series and the different functions this entails. Neither Goethe nor the other "Naturphilosophen" put forward any theory of evolution, but their conception of the transformations of primordial types went some way towards preparing men's minds for such ideas. Nor must we forget that Goethe invented the word *morphology* for a branch of science that was later to play so conspicuous a part in connexion with evolutionary theory.

The Cuvierian era was also notable for the advance of embryology. Through the work of v. Baer and others the process of fertilization was coming to be grasped, and the doctrine of Preformation was finally exploded. Very important too was the formulation of v. Baer's *Biogenetic Law*, which, as Singer has pointed out, consists, in effect, of four propositions:

(1) In development, general characters appear before special.

(2) From the more general are developed the less general, and finally the special.

(3) In the course of development an animal of one species diverges continuously from one of another.

(4) A higher animal during development passes through stages which resemble *stages in development* of lower animals.

The formulation of such general principles awakened in these studies an interest which was soon to receive an enormous stimulus.

Meanwhile in another branch of inquiry there was growing up a point of view which was destined to exert great influence in bringing about the Darwinian era. Geologists were shaking themselves free of the "catastrophes" which Cuvier had imposed upon them. In 1830 began to appear Lyell's *Principles of Geology* with its insistence upon the uniformity of geological succession. By abolishing the catastrophe the geologist brought the naturalist face to face with the problem of explaining the connexion between the fossil forms of life and those still living. As the science of palaeontology developed, and fresh discoveries were made, it came to be more clearly seen that the distribution of these fossil forms in time accorded well enough with the idea that there existed a genetic continuity between them, while it was not easily to be reconciled with any other hypothesis.

As the Cuvierian era proceeded, the position of the great French naturalist—the orthodoxy of the period—was being steadily undermined. The growth of embryology, of morphology and of palaeontology was telling more and more against the doctrine of the fixity of species, and pointing with increasing emphasis to the existence of a genetic continuity among living things. This involved the conception of the mutability of species through some process of *evolution*, a word first used in this connexion by Herbert Spencer in 1852.

* * * *

Indeed the arguments for such a process were marshalled by Robert Chambers in the anonymously published *Vestiges of Creation* which appeared in 1844. Not only does he bring forward those from the sequence of types in palaeontology, from homologies in Vertebrates and from the Biogenetic Law, but he also draws attention to two further lines of argument which Darwin later developed with great effect. First, he points to the existence of the various domesticated races of animals as evidence that certain species, at any rate, are capable of modification, and that such modification can be transmitted: secondly, he adduces the existence of rudimentary organs as an argument against the hypothesis of special creation. Structures such as the small teeth in the foetus of the whalebone whales, the small imperfect additional toes on the splintbones of the horse, the traces of hind limbs in certain snakes, become both intelligible and instructive on the hypothesis of a genetic connexion between the different forms of animal life.

* * * *

The *Vestiges* was a widely read book, passing through many editions in a few years. Nevertheless, it cannot be said to have exerted any marked influence on the scientific minds of the time. It did not get the serious consideration which it deserved in spite of obvious shortcomings. The author was not a trained biologist, and as Darwin wrote in a letter to Hooker "his geology is bad and his zoology worse". This doubtless told against him, so that men of such diverse minds as Sedgwick and Huxley reviewed the book in crushing manner. Nevertheless, Darwin later paid it the tribute of having "done excellent service in this country in calling attention to the subject, in removing prejudice, and in thus preparing the ground for the reception of analogous views". The popularity of the *Vestiges* is evidence that the minds of educated men were not averse to the idea of the mutability of species. Its failure to impress the scientific world, apart from the crudity of its science, probably lay in the fact that it offered no plausible suggestion as to the manner in which the genetic succession of forms could have been brought about. It was the suggestion of such a factor—Natural Selection—by Darwin and Wallace that led to the immediate acceptance of the evolutionary idea. It is true that the publication of *The Origin of Species* excited immense opposition. But that opposition came almost entirely from outside the ranks of science, and the argu-

ments used were largely of the nature of appeals to existing prejudice, particularly where the status of man himself was concerned. The immediate support for the new views forthcoming from Lyell, Hooker and Huxley led rapidly to acceptance by the scientific world. The need of some synthesizing idea bringing into relation with one another the great body of facts derived from palaeontology, morphology and embryology had long been felt, but their synthesis through the idea of evolution had been delayed through ignorance of any factor by which such evolution could be supposed to have been brought about. That factor was now supplied, and botanists and zoologists at once set out to rearrange their facts and systems in the light of an evolutionary succession of life. For several decades their main interest lay in the construction of genealogies relating together the great groups of the animal and vegetable kingdoms. Among animals three of these great groups—Cuvier's Vertebrata, Mollusca and Articulata—were obviously pretty homogeneous, and within them the refined study of adult morphology and palaeontology were largely relied on. It was otherwise with Cuvier's 4th Embranchement of Radiata, and with the miscellaneous class Annélides. Small, and often insignificant forms, they had been far less studied. But with the illuminating idea of evolution they offered a scarcely explored field where facts of the highest interest might readily turn up.

* * * *

It was among these lowly creatures that the evolutionary morphologist might expect to find forms of life which would shed light upon the relations of the great homogeneous groups with one another. He turned to the task with enthusiasm, and with the aid of the microtome and of vast improvements in microscopic technique was produced the prodigious volume of morphological research which crowds the journals of that time. But perhaps the most characteristic type of research of the period is that known as Comparative Embryology. The idea upon which such work was based arose from the impact of the evolutionary idea upon the Biogenetic Law. The result was the Recapitulation theory, which stated that the ontogenetic history of the individual was a repetition, often blurred and abbreviated, of the phylogenetic history of the group to which it belonged. It received an immense stimulus from the brilliant work of Kowalewsky on the Tunicata, or sea-squirts, a group hitherto placed among the Mollusca. Through the study of their embryology Kowalewsky was able to show that in the course of their development they pass through stages conforming to a primitive vertebrate plan, with a free-swimming tadpole-like larva. This work led to a reconstruction of the genealogical tree of the great vertebrate phylum, taking its origin back to a simple hypothetical form without any vertebrae at all.

* * * *

A few years later Bateson showed that the peculiar worm *Balanoglossus* passes through morphologically comparable stages and should therefore be regarded as having sprung earlier from the same stem as that which later gave rise to the Tunicata, to *Amphioxus*, and to the Vertebrates. Further, it was known that other members of the *Balanoglossus* group passed through an apparently quite different life history with a free-swimming larva resembling that found in certain Echinoderms. From careful morphological investigation, guided by generalizations such as the germ-layer and the coelom theories derived from other forms, it was shown that the two kinds of development could be reconciled with one another. So was the conclusion reached that mammals and starfishes had sprung from the same primitive stock, though separating very early from each other and pursuing very different evolutionary paths. What Comparative Embryology was doing for the Vertebrates it was doing also for every other group in the animal kingdom, and it was not long before an enthusiast such as Haeckel could confidently state the

evolutionary history of any form of life, however complex, in its lengthy passage from primordial slime. In the pursuit of these ideal genealogies there was no check upon the imagination: for the only branch of science that could have supplied it, viz. palaeontology, was from the very nature of the case unable to do so. These far-away small soft things could have left no record in the ancient rocks. So through the first few decades of the Darwinian era the Comparative Embryologist passed from triumph to triumph. Balfour's *Comparative Embryology* issued in 1880–81 is a landmark in these studies, and the traditions there established were carried on by Ray Lankester in England and by Korschelt and Heider in Germany.

* * * *

Darwin's great book was entitled *The Origin of Species*. Its thesis amounted to a denial of the existence of species as hitherto understood. What we term a species is merely a time concept—a cross-section at a given moment through a gradually changing and genetically connected series of life forms. Though the change may be imperceptible to our appreciation a species is different from what it was yesterday, and will be different again to-morrow. With the doctrine of the continuity of genetical succession it was deprived of that character of fixity with which it had been endowed by the theory of special creation. Curiously enough, as Bateson pointed out, the systematists, the very people on whom the new doctrine might have been expected to exert the greatest influence, stood obstinately aloof. Though they might pay lip homage to evolution, they continued unmoved to discuss what was and what was not a "valid" species. The men whose work brought them into closest contact with species treated them as realities, and with the conviction of some degree, at any rate, of fixity about them.

Thirty-five years after the *Origin* had appeared a new note was struck when, in 1894, Bateson published his *Materials for the Study of Variation*. Darwin's theory had been based in part upon what was termed the Principle of Variation. No two living things were exactly alike and their differences were what Natural Selection worked upon to bring about the gradual change of form we term evolution. In the absence of exact knowledge the phylogenetic architect had assumed the existence of unlimited variation in all directions for the construction of his genealogical edifices. Bateson protested that after all the nature of species had *not* been solved; that the forms of living things remained "specific" in spite of the doctrine of continuity in genetical succession.

Some other means of attacking the age-old problem must be found, and he suggested that the fairest hope of success lay in the study of the nature of variation itself. We must discover what kinds of variation do actually occur in Nature, for these are the limitations by which evolutionary change must be bounded. From his collection of facts he showed that variational change is in large measure *discontinuous*—that in a freely interbreeding community may occur definite varieties without the existence of any series of intermediate forms to link them with the normal.

May not this demonstrable discontinuity in variation lie at the root of the discontinuity which is so characteristic of closely allied species? Bateson's call to a new study excited but little interest, and in a few years his book was to be found among the list of cheap remainders. Orthodox complacency required a further stimulus, and this was soon to be provided in de Vries's *Mutationstheorie*. The publication of this work in 1901, coinciding as it did with the rediscovery of Mendel's paper, may be said to mark the close of the Darwinian and the opening of the Mendelian era.

* * * *

We must now turn to another branch of inquiry which, at first seemingly independent, is now playing a part of the highest

importance on the question of the nature of species that Darwin had brought into such prominence. Following Schleiden's historic paper of 1838 the cell doctrine became firmly established by the work of Kölliker and others during the next two decades. Improvements in microscopic technique led to rapid development in our knowledge of the nucleus, and by the middle of the Darwinian era, chiefly through the researches of Strasburger and Flemming, the complex processes of cell division were unravelled, and the chromosome stood ready to assume the important role that was later to be thrust upon it.

* * * *

It was some time, however, before it was drawn into the orbit of the species problem. Beyond the fact that their number was constant for all the cells of a given species little was yet heard of chromosomes in this connexion. It may, however, be mentioned that this more precise knowledge of the mechanics of cell division now available served to place a curb on those speculations which attempted to analyse the cell into smaller hypothetical units in attempts to explain the phenomena of heredity. One cannot imagine Darwin putting forward his theory of "gemmules" had he been acquainted with the nature of cell division.

The Darwinian theory of evolution through natural selection had been built upon the twin pillars of Variation and Heredity. The continuous nature of variation, so essential to the theory, was, as has been pointed out, challenged by Bateson in 1894. We may now turn for a moment to heredity. Darwin frankly confessed that very little was known about the subject. Speaking generally he and his followers subscribed to the popular saying that "like begets like", though with some reservations. The general rule might be upset through the operation of some unknown factor to produce *reversion*, where progeny "throw back" to some remoter ancestor instead of closely resembling their parents. Again, the unaccountable "sport" that might at times arise was generally referred to some abnormal environment, such as domestication. But the "like" begotten by "like" is never exactly alike. There was supposedly some range in variation upon which natural selection could work. It was all rather vague, but sufficiently in keeping with popular opinion to excite little criticism on the part of the scientific, whose ignorance in these matters was on a par with that of the crowd. The stock-breeder and the fancier could have told another story, but with the publication of *Animals and Plants* a few years after the *Origin* the scientific world ceased to take any interest in their observations. The matter was regarded as closed for, as a distinguished zoologist remarked, "Darwin had swept the board." It is a curious fact that the publication of Darwin's work, instead of stimulating, should have killed all interest in breeding research. Nevertheless, an effort on quite different lines was made to clarify ideas upon heredity. Originating in the fertile mind of Francis Galton it consisted essentially in the application of statistical methods to the study of resemblances between groups of relatives in different degree. On the basis of specially constructed correlation tables for this or that chosen character, Galton formulated the so-called "Law of Ancestral Heredity" which stated the *average* contribution made to an individual by parents, grandparents and remoter ancestors. This essentially quantitative view of the subject was enthusiastically taken up by the mathematician, Karl Pearson, and at the close of the century led to the foundation of the Biometrical School. Based upon the view that all variation is continuous and equally transmissible, it rapidly collapsed when Mendelism proved these views to be untenable.

* * * *

One further attempt at understanding the nature of heredity should be mentioned here. This was August Weismann's famous germ-plasm theory. Though entirely specu-

lative it was the product of a trained biologist who in earlier years had made important contributions to science. His theory emphasized the continuity of the germ-plasm through successive generations. The body, or soma, was merely a temporary structure formed by the germ-plasm for its own conservation. Their function of conserving and passing on the germ-plasm fulfilled, bodies wore out and died, but the germ-plasm was immortal. The form taken by the soma depended primarily upon the germ-plasm which formed it, and though it might be altered by external conditions during its lifetime such alterations could not be transmitted. There was no room in the theory for the transmission of somatic alterations in the Lamarckian sense, and Weismann utterly rejected them. By demanding evidence and finding none that would stand criticism he did a great service to biology. But it must be confessed that in so doing he tended to weaken the Darwinian position. Although Darwin himself regarded natural selection as the main factor in producing evolutionary change, he attributed much to the influence of the environment and to the Lamarckian factor of "use and disuse". Weismann swept all this away and saw in natural selection the only factor for bringing about evolutionary change. Variation for him was initiated in the germ-plasm—partly through *amphimixis*—the mingling of two germ-plasms that occurs at every act of fertilization—and partly through a selective struggle in the germ-plasm itself among the complex system of "biophors, ids and idants" of which it was composed.

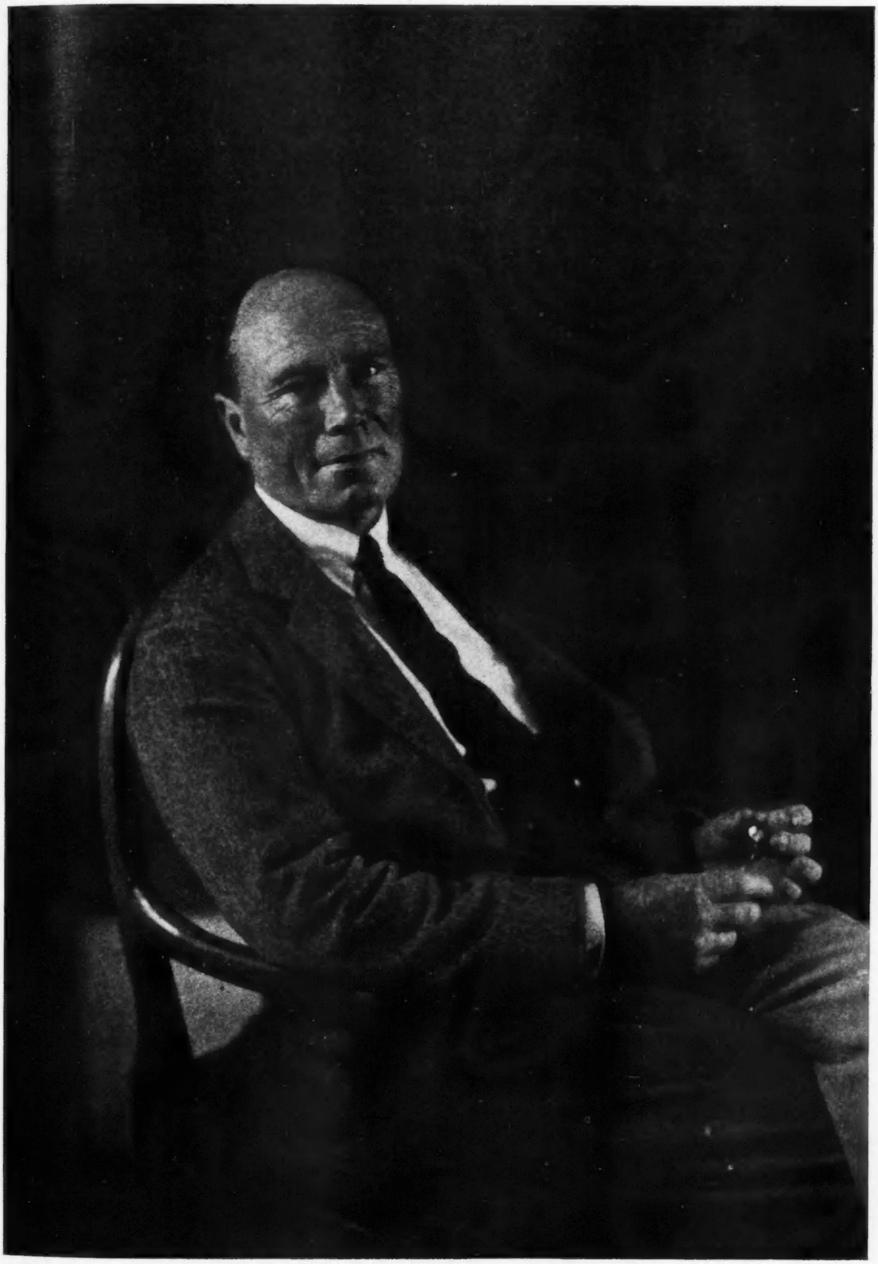
* * * *

The fact that Weismann localized his system of hereditary units in the chromosome and so brought his theory into consonance with the growing science of cytology had much to do with the influence that Weismannism undoubtedly exerted. But by cutting away the Lamarckian and environmental props and by insisting upon the "omnipotence" of natural selection in evolutionary change he threw upon this last factor a burden which, in the opinion of many, was too great for it to bear.

Darwin was a philosopher, but at the same time he was a naturalist—one of the greatest. In the former capacity he wielded enormous influence over the laboratory, by stimulating those researches into Comparative Embryology which may be said to strike the dominant note of the era. As a naturalist also his influence on the period was very considerable, and principally in the study of what has been termed Adaptation. That animals and plants are on the whole peculiarly adapted to the circumstances in which they live had long been recognized. The pious who were also naturalists had found in these adaptations further evidence for the power and beneficence of the Creator who in the beginning had taken thought for all these things. Such was the line adopted by John Ray in his work on *The Wisdom of the Creator* which appeared in 1691.

* * * *

The promulgation of the idea of natural selection at once vested these inquiries with fresh interest. On the Darwinian view any kind of variation which gave to its possessor even a slight advantage in the struggle for existence would gradually accumulate as the generations passed. Taking natural selection for granted one must suppose that the various characters of animals and plants have come to be what they are because they are *useful* to the individual in the struggle for existence. Hence if a character can be shown to be useful its very existence bears witness to the efficacy of natural selection. The argument is somewhat circular, and I am not defending it. But it is the sort of argument that was at the back of the minds of those who turned to the study of adaptation. In his books on *The Forms of Flowers* and on *The Fertilisation of Orchids* Darwin himself had blazed the trail. As the result of numerous experiments with plants he had come to the conclusion that self-fertilization was injurious, a thing



PROFESSOR R. C. PUNNETT

abhorred by Nature. As the result of numerous observations he had noticed that many flowers are so constituted as to be more readily fertilized by foreign pollen arriving through insect agency than by the pollen they themselves produce. By allowing and often even insisting on, cross-fertilization, the form of the flower must be regarded as an adaptation beneficial to the species. For the progeny so arising would be more vigorous and better able to withstand competition. Such a useful adaptation might well be brought about through the operation of natural selection.

* * * *

The study of Adaptation offered a pleasant alternative to the discipline of the laboratory, and many were those attracted to it. On the animal side were recorded innumerable cases of "protective resemblance"—a butterfly resembling a leaf, a spider simulating an excrement, and so forth. In most cases the description wound up with a few remarks on the efficacy of natural selection. In Ray's time it had been the Omnipotence of the Creator. Very seldom was any effort made to test whether the case described, e.g. a variation in colour, *was* of any value to its possessor; whether those in possession of the presumably advantageous variation *actually did* have any advantage as judged by survival value. And that of course is what is wanted.

Among the phenomena of Adaptation none has excited greater interest than that of Mimicry. In its special sense the term is applied to cases where a species, generally an insect, resembles in appearance another species which may belong to a different family, or even order. Such resemblances were first noticed by Bates when collecting butterflies in South America. The *Origin* had just been published, and in 1862 Bates formulated a theory to account for these resemblances in terms of natural selection. He had observed that in the cases he had come across one of the forms, and that the more plentiful, was not attacked by birds, and this he attributed to its possessing a disagreeable flavour. It was also characterized by a conspicuous colour pattern. This was supposed to have developed through the operation of natural selection, for it was of advantage to its possessor in advertising its unpleasant properties. After a trial or two the would-be predator would associate the striking pattern with an unpleasant flavour, and would henceforth leave it alone. The more striking the pattern the more efficient the advertisement—a point which was seen to by natural selection. The development of such a striking pattern, or "warning colouration", was the first stage in the production of an example of mimicry. Bates observed that the other species concerned belonged to a group which might be preyed upon by birds, since it had not been able to develop a nauseous flavour, even through the operation of natural selection. Careless in this respect natural selection came to its rescue in another way. By favouring those variations in which it approached the pattern of the nauseous "model" it gradually built up a similar pattern in the palatable "mimic". Henceforward the mimic enjoyed immunity from the attacks of the postulated predator, which had learned to associate the pattern with unpleasant qualities.

* * * *

The argument was accepted by Darwin, who devotes several pages to these mimetic resemblances in the fourth and subsequent editions of the *Origin*. Later, Wallace and Trimen recorded further instances of mimicry among Oriental and Ethiopian butterflies, and since that time large numbers of cases have been described, chiefly from tropical countries. Since its discovery mimicry has always been regarded as of the very first importance by the advocates of natural selection as *the* factor in bringing about evolutionary change. Weismann regarded it as the keystone of the arch. As he and those who think with him would say—Here we have all these peculiar cases of

special resemblance, as to the reality of which there can be no doubt; on the theory of natural selection alone are they comprehensible, and no other plausible explanation has ever been adduced. Throughout the Darwinian era and beyond it the subject of mimicry excited the keenest interest. Around it there has grown up a voluminous literature, though this, as Singer justly remarks, is peculiarly naïve and unscientific. For the critical mind there are many reasons for dissatisfaction with the theory as it stands. Much that it postulates is utterly at variance with the established results of genetical research. Yet the existence of these peculiar cases of resemblance remains one of the strongest and most fascinating problems of the naturalist. Were it to be solved much else would be solved also. But the solution is still to seek.

Wallace, as we have already seen, was one of the pioneers in developing the theory of mimicry. He was also a pioneer in another line of study with an intimate bearing on the doctrine of evolution—the study of the geographical distribution of living forms. Darwin himself had earlier drawn attention to the peculiar nature of the fauna and flora of certain islands. In *The Voyage of the Beagle*, when discussing the collections he had made in the Galapagos Islands, he pointed out that not only were many of the species peculiar to the group, but that each individual island contained forms which were not present on the others. Yet, though distinct, they were not very different from one another.

* * * *

Why had it been necessary to create all these slightly differentiated and narrowly distributed species? That one species should have been created for each small island seemed hardly rational; but how then had these different species arisen and why did they belong to South American genera? It was largely this problem of island forms, as propounded by those of the Galapagos Archipelago, that set Darwin's thoughts travelling to the problem of the formation of species. With the formulation of his theory, this group of facts, like so many others, seemed to find a rational explanation. The subject was later developed by Wallace in greater detail. He evolved a geographical system for the distribution of animals which has stood the test of time. The nature of the fauna of an area is largely wrapped up with the geographical changes that the area has undergone. If these changes are taken into account, and if we assume an organic evolution from more primitive to more specialized forms, then the geographical distribution of animals becomes intelligible. New Zealand contains no indigenous terrestrial mammals because these had not yet made their appearance when geological conditions led to the isolation of that country from continental land. Australia lacks indigenous placental mammals because it received its mammalian population when the more primitive marsupial alone existed. Before the placental evolved elsewhere Australia had become cut off from the rest of the world.

* * * *

Provided that the postulated changes in land areas are confirmed by the geologist, and that the phylogeny assumed agrees with that of the morphologist, the fact that an explanation of the past and present distribution of animals is possible is definitely in accordance with the theory of organic evolution. That both geologist and morphologist have in the main concurred is strong circumstantial evidence for the theory. As to the manner in which this evolution has come about it offers no evidence and, for this reason perhaps, it remains, in spite of accumulated data, in much the same position philosophically as that in which Wallace left it. But it is not unlikely that fresh interest will develop in it when we are in the possession of more precise knowledge of the manner in which variations arise.

Most of the lines of research actively pursued in the Darwinian era—morphology, comparative embryology, palaeontology, geographical distribution, adaptation—

were lines of inquiry directly stimulated and largely remodelled as a consequence of the Darwinian doctrine. Others like cytology lay outside its orbit, and to these we may add experimental embryology, or, as some prefer to call it, developmental mechanics.

* * * *

Initiated by Wilhelm Roux in the 'eighties the new line of study was definitely experimental, seeking by such means to understand the forces at work in the embryo. Hitherto the embryologist had been content to describe the normal course of development, and to draw from it phylogenetic inferences. For the newer school phylogenetic problems presented no interest. It was the aspect of organization that attracted them, and they sought to throw light upon the causes of development by studying it under abnormal conditions, such as mutilation, distortion, changes of chemical or physical environment, etc.

In the hands of Driesch and others, especially Jacques Loeb whose experiments on artificial parthenogenesis excited much interest, this new line developed rapidly, particularly in America. Historically it is of interest in that its exponents broke clean away from the Darwinian tradition. Hitherto, under the denomination of the Biogenetic Law, the *cause* of developmental sequence, in so far as embryologists concerned themselves with such things, lay in phylogenetic history. Many Crustaceans developed a nauplius larva at some period of their ontogeny because, in the remote past, they had all descended from a nauplius-like ancestor. When the nauplius larva failed to materialize it was because the developmental sequence had been abbreviated through natural selection; and in such cases it was the self-appointed task of the embryologist to detect this masked nauplius stage and so to relate the abbreviated to the fuller sequence.

And there the matter ended. The aim of the developmental mechanists was to inquire how far embryological sequence could be interpreted on purely physical grounds. In one respect, however, the new line had considerable influence on Darwinian studies. By opening up a fresh and independent avenue of research it attracted away the rising generation of embryologists, and the deflection of ideas and aims contributed largely to the wane of morphology towards the end of the nineteenth century.

And now arose another line of study by which the Darwinian doctrine was to be tested. I refer to the experimental study of heredity which suddenly came to the fore and the rediscovery of Mendel's forgotten paper in 1900. Though, properly speaking, this movement lies just outside the Darwinian era as defined above, it is in some ways so intimately associated with it that I may perhaps be pardoned if I speak briefly of its impact on the older doctrine. For the evaluation of any doctrine we ought, if possible, to consider it in relation to what came after as well as to what went before.

We have already seen how, in 1894, Bateson had challenged the idea of continuous variation implicit in the Darwinian teaching; how he had pointed out that variation might be discontinuous—that two well-marked variants might be found among a freely breeding population without the existence of those intermediate forms which, on the accepted doctrine, should occur. Moreover, he had suggested that such discontinuity in variation might be at the root of the discontinuity of species. Might not species be real and permanent things sharply marked off from one another even as the systematist averred, and not mere cross-sections of a gradually changing life-sequence as the Darwinian theory supposed?

* * * *

Convinced of the existence of discontinuity in variation Bateson set in train experiments to discover how such variations behaved in the hereditary process. He had not, however, proceeded far when the news of Mendel's work reached him.

Discontinuity in Variation had its counterpart in the Discontinuity of Heredity. This brought with it the conception of definite entities—factors they were then called—in the germ-cell corresponding to definite characters in the individual. Moreover, these factors were conceived of as, in general, permanent things passing unchanged from gamete to zygote and from zygote to gamete for innumerable generations. Upon them depended the characteristics of living things. The claim was no longer that variation *might* be discontinuous, but that all heritable variation was in its nature essentially discontinuous. Such a claim was at once resisted by the Biometricians, for they saw that its acceptance knocked from under them the foundation upon which they were building. The controversy that ensued was short but decisive. The first impact of Mendelism on the Darwinian doctrine was to discredit the current view of the nature of variation. Begun under these auspices the experimental study of heredity went rapidly ahead. Its adherents were too actively engaged to pay much attention to relating the new knowledge to evolutionary doctrine, though incidentally the phenomenon of reversion on crossing, which had so puzzled Darwin, received an explanation. Attention was largely focused on the problems of linkage and of sex.

* * * *

So went by a decade, and then came into the picture the little fruit-fly *Drosophila*, and with it came the *gene* theory in which the *factor* of earlier workers was re-christened in good American and assigned a definite habitation in the chromosome. Research has now made it certain that the genes, upon which depend the manifestation of characters in the organism, are bodies of a size calculable within reasonably narrow limits and are arranged within linear series in the chromosomes. The material basis of heredity was laid bare and the gene with its predictable behaviour and ascertained position in a visible structure has taken the place of the purely hypothetical gemmule, pangen or biophor of earlier speculation. Thus was established that intimate contact between genetics and cytology which is now the feature of modern work.

* * * *

It is unusual to find two branches of study begun, and for some time pursued, entirely independently, fuse together as it were into a single whole. Yet to-day neither cytology nor genetics can be profitably studied apart. A peculiarity in chromosomal arrangement is at once reflected in some peculiarity in heredity, and, conversely, seemingly abnormal transmission at once leads to an examination of the chromosomal complex. Quite recently the pioneer work of Muller in exposing *Drosophila* to the influence of X-rays has demonstrated that not only can fresh and unknown mutations be artificially produced, but that through genetical analysis the nature of the chromosomal changes involved can be discovered. As might be expected the new technique is producing its most striking results in the more easily worked plant with its simpler structure. Notably in *Datura*, *Crepis* and *Nicotiana*, genera already largely analysed from the genetical standpoint, have the results been most revolutionary. For it has been found possible to produce new forms of plant life, with definite characters of their own, breeding true to these characters, and at the same time showing sterility towards the parental forms.

In other words there have been created new species, the essential nature of which, as compared with the parental forms, is a rearrangement of chromatic material, a realignment of the genes, accompanied perhaps by losses or additions of some or others among them. Although this stage has not yet been reached in animals the evidence from the study of the chromatic material in closely related forms of *Drosophila* points in the same direction.

That all this must necessarily lead to a recasting of our ideas concerning the nature

of species and their mode of origin is obvious; for to-day we are in possession of vital knowledge which was lacking in the Darwinian era. Understanding, as we do, much of the process of heredity and something of the nature of variation we view with altered vision the age-old problem of species. That their existence is based on some mutational process we are convinced, and we know too that there is every prospect of gaining an insight into that process in the laboratory. But at present we do not know for certain whether this process is necessarily *extrinsic*, whether mutations are always due to the action on the germ-cells of some force external to the organism. There is always the possibility that there may be some form of *intrinsic* mutation, dependent upon some part of the highly complex cell mechanism failing to keep step, as it were, over a long stretch of countless cell divisions, and thereby leading to the formation of something fresh, should it prove to be viable. Mere speculation as this is, it may serve to emphasize that we are as yet in no firm position to hazard conjectures on the origin of species in spite of the fact that we have learned a great deal about their nature.

Meanwhile, in concluding, we may take a brief backward glance over our allotted period. What have we taken from it, and what have we discarded? In the first place we still hold by the theory of evolution, regarding the world of living things as a dynamic, and not a static concern. The idea is of course an old one, and Darwin's chief glory, as Butler said, is not that he discovered it but that he made men believe in it—and what glory, he added, could be greater than this? Natural Selection also is still with us, but in a rather different sense. For Darwin and his followers, owing to their conception of the nature of variation, it was in large measure a *creative* force, accumulating small variations until they attained a magnitude that enabled them to play a part in specific change. Our insight into the nature of variation has changed all this. Natural Selection, said Bateson, is a true phenomenon, but its function is to select. It plays the part merely of a selective agent on heritable variations, which have already arisen through an independent process of mutation, conserving the beneficial and rejecting the inimical, while producing no effect upon those that are neither the one nor the other. Through such limitations of its scope we are released to-day from the necessity of finding a use in everything merely because it exists. On the other hand continuity in heritable variation has gone, and with it the idea of continuity between species. Species are once more sharply marked-off things with hard outlines, and we are faced once more with the problem of their origin as such. The idea of yesterday has become the illusion of to-day; to-day's idea may become the illusion of to-morrow. "For", says Meredith, "the mastery of an event lasteth among men the space of one cycle of years, and after that a fresh illusion springeth to befool mankind." Doubtless many masters of the event will follow after Darwin and Bateson in wielding the Sword of Aklis, and through the dispelling of illusion after illusion mankind may eventually encounter the ultimate residue, perhaps the ultimate of all illusions, which we optimistically designate as truth.

The Fighting Fish

By Dr Franz Graf Zedtwitz

THE natives of the Sunda Islands rear a splendid small fish (*Betta splendens*) for sport. These are fighting fish, which are peculiar in that they cannot bear the presence of another of their kind. The males attack as soon as they see each other, dashing in over and under and revealing their brilliant red and blue colouring. They snap fiercely and when they get a grip they bite until the opponent is killed. The natives when matching the fish for a fight interfere before one kills the other.

The male collects the eggs from the bottom

These fighting fish are so attractive that in Europe they would be gladly kept in aquariums. They breed freely. There are various types of fighting fish but the most beautiful are the veiled fighters with their long silky fins, which the males when enraged raise up erect. There are specimens in white, red, blue and green.

Mating is a quaint process. First of all the male comes to the surface and snaps at the air, which he quickly exhales again. This process he keeps up until a thick nest of foam is formed about the size of a man's hand. This accomplished, he seeks to drive a

female under the "nest", and displays all his finery to attract the bride. The eggs when laid fall to the bottom where the male gathers them in his mouth and spits them out into the nest of foam. He has to be careful, because the female if not watched will eat up the eggs as she has no interest in them, for in all probability those the male gathers up do not belong to her. Having collected the eggs in the nest, the male watches over them for twenty-four hours when the young, perhaps 200 of them, are hatched.

(Above) The foam nest seen from the surface.

(Left) The male displaying.

(On the opposite page) The male constructing the foam nest, and (right) a courtship attitude.

Still Unexplored

By Sir Hubert Wilkins

(Explorers are not yet without value, for more than two million square miles are still unexplored on the earth)

IS the field of discovery still sufficiently vast for explorers of 1938—Are there still unknown lands for them to penetrate? And, when they leave in search of adventure, can they still hope to discover some new race, or a region of new wonders?

Yes—and for a very long time yet.

A number of us, explorers, have estimated that there are still over two million square miles of unexplored territory on this planet. Which are the continents that have not yet revealed their secrets to mankind?

There is, first of all, South America, of which the topographic maps will not be completed for another twenty years. The only existing information on the South American hinterland is that which was gathered by Captain Faurett. In this region, there still exist vast tracts and peoples entirely unknown to civilization. This continent alone is sufficient to keep all the explorers of the world busy.

The extreme south of America is also partly unknown. What can one think of this country where, it appears, certain tribes have never even seen a civilized man? It must be a magnificent field for exploration.

After South America, Asia is still the continent that holds the greatest mystery for visitors. So far, expeditions to the summits of the Tibet and Gaurisankar mountains have had little more than sporting results. Beyond these chains of mountains lie vast areas on the borders of Turkestan and of which nothing precise is known. There are no maps nor geographic records to give one an idea.

Thus there are vast expanses in this world about which maps are silent. There are about a dozen of these areas of which we know nothing. It is known that, in Central Tibet, certain prehistoric villages, mentioned in ancient documents, are still to be found. But it has not yet been possible to go beyond certain boundaries of Tibet. Although it may appear incredible, there are still villages in these regions that have never had any contact with the civilized world.

Each year, a great many explorers launch an attack on this unknown world; but even with the modern material now obtainable, certain summits remain unconquerable. Dr Walter, who took part in the last expedition to the Himalayas, recently assured me that we must wait until 1945 before we can learn anything of Central Tibet.

On the other hand, Europe holds no further secrets, although it is not so very long ago that certain parts of Northern Russia had still to be surveyed with precision.

There are also the two Poles, which will certainly not be fully charted for many years yet. The black continent no longer has any white patches: the smallest Oceanic islands are all known—but the Poles...! All those who have gone beyond 75° of latitude know what that means.

In spite of Amundsen and many others, the Poles are a mystery. Although the North Pole is now almost entirely charted, many points in connexion even with the North Pole remain unexplained, and ethnology has not been able to shed light on the curious and archaic life of some of the Samoyeds. Still less is known of the South Pole. Thirty years of work and research will be necessary before certain Polar limits can be quite established.

Men of quality are not lacking for this purpose. On this account, humanity need not complain. But, paradoxical as it may seem in this age of aviation, submarines, wireless, and television, we still lack the necessary equipment for exploring our Earth.

Special apparatus for the sole use of explorers exists, but it is always very expensive. Perhaps one need look no further for the reason of our delay in learning about the world.

Maxwell's Demon

By L. INFELD

WHAT is civilization? By what criterion can we judge one civilization higher than another? This seemingly simple question is indeed difficult to answer. From a purely material point of view we could, perhaps, choose some standard by which to measure degrees of civilization. We can, for instance, compare the rates of production and consumption of a certain commodity in, say, two different countries. Here, however, we shall restrict ourselves to the simpler aspect of this problem, to the *physical* one. Instead of coal-mining, the manufacture of soap or dynamite, the building of hospitals, schools or military barracks, we shall consider something very much simpler.

We half fill a glass with black ink and then add an equal quantity of water. A simple phenomenon occurs. The water and the ink mix and, after some time, form an apparently uniform liquid. Whilst watching the liquids mix we can ask two fundamentally different questions. The first is primitive: how to accelerate the mixing process? This is easily answered. Stirred with a stick the liquids mingle more rapidly. The second question is more sophisticated. The ink and the water have already begun to mix. How can this natural process be undone? How to return to the stage when one half of the glass contains water and the other half ink? We see immediately that this is a more complicated problem. From the kinetic theory of matter we know that both the particles of ink and the particles of water are in constant motion. They move straight ahead, and, sooner or later, collide with each other. Each collision causes a change in the direction of motion. Thus the two kinds of particles intermingle and the two liquids are blended into one. This is known as the phenomenon of diffusion. The particles of liquid speed about in all directions, entirely at random, and their motion is governed by probability laws. In principle, strange things might happen. At a given instant, for example, all the particles of water might rise towards the top of the glass, whilst those of the ink sink towards the bottom. Although such an occurrence is, in principle, not impossible, it is, however, highly improbable. Similarly—this example is chosen at random—a monkey haphazardly tapping the keys of a typewriter might conceivably write out some famous poem. It is, of course, extremely doubtful that this would ever actually occur. Instead of finding out how to divide the mixture into water and ink, we might just "wait and see". If we could wait milliards and milliards of years perhaps one day we might see some change which would cause the liquid to separate into its two components of ink and water. But since the span of human life is limited, we had better consider some other way out. Here we turn to the concept of Maxwell's demon. This is a physical fiction named after one of the greatest physicists, the creator of the electromagnetic theory of light. He was also the first to apply statistical methods to physical research.

What is Maxwell's demon?

Imagine that two hundred invitations to a meeting have been accepted. One hundred white and one hundred black guests arrive. These people are shown into two rooms, room W (white) and room B (black).* The representatives of the different races mingle freely in the two rooms. We

* This example implies no racial prejudice on the part of the author!

now wish to divide the company so that there are only white people in room W and only black in room B. So far the guests have been wandering around at will, in and out of both rooms. If we now station a porter by the door dividing one room from the other, he will play a similar role to that of Maxwell's demon in separating ink from water. Suppose now that a negro leaves room W to saunter into Room B. The porter takes no notice of him. Similarly, if a white man goes from room B to room W the porter does not interfere. But, should a white man try to enter room B or a negro room W, the porter will politely but firmly close the door in his face. Once a white man is in room W or a negro in room B he must stay there. The porter never uses force. He simply opens and closes the dividing door at exactly the right moment, i.e. he allows the guests to pass only into the room in which they belong. The guests' movements are entirely casual and the porter may have nothing to do for some time. It may happen that the guests wander only in the right directions and the porter will not have to interfere. In any case, we can see, after a sufficiently long interval, the results of the porter's efforts. If we assume that the guests are continually coming and going, room W will, sooner or later, fill with white people and room B with black.

The fiction known as Maxwell's demon plays a similar role in separating ink from water. Imagine a surface, impervious to either of the liquids, dividing the vessel into halves. In the centre of this partition we picture a very tiny hole. Imagine further that Maxwell's fantastic demon opens and closes this hole according to the direction of motion of each particle of ink or water. The demon behaves towards the particles in the same way as the porter towards the guests. The motion of the particles is quite casual but the hole is opened and closed strictly according to the direction in which they are moving. Finally, the ink will be separated from the water and nature's work of levelling out differences undone.

Nature continues her levelling process unceasingly. The sun and the stars are continually sending their radiation into space, thereby weakening their energy. Reservoirs of heat grow cold unless they are fed. Rocks are gradually worn away by the elements. The ebb and flow of the oceans is exhausting the energy of our planet. Motion which is not influenced by some force slows down and stops owing to the friction and resistance which always accompany it.

Man must do his best to counteract nature's levelling process. To live, to build up centres of civilization, he must create special artificial systems such as: reservoirs of energy, drastic changes of temperature, high voltage currents, gas containers with very high and very low pressure, pure chemical bodies. He must create just such systems as those that nature strives to destroy because they disturb her levelling activities.

The above example of separating ink from water shows most simply the undoing of nature's levelling work. It is a physical example of the function of civilization. The actual division by opening and closing the door does not call for work in the physical sense of the word. After all, what is it for a demon to open and close a hole, even a milliard times a second? But other things are needed; the ability to distinguish particles of ink from particles of water; rapid and clear thinking; in fact, the demon must be *intelligent*!

There is eternal struggle between nature with her levelling activities and man building up civilizations, creating ever more artificial and unnatural systems. Nature must and will win in the long run. Her destructive work is bound to end in the downfall of civilization and all man's achievements. The price of our temporary victory is the inevitable defeat of some future generation.

Nutrition and Health

II. What are the Constituents of Food?

(This is Mr March's second article on Nutrition; a further article will appear next month)

By C. H. March

TO carry out any form of work at all you move your muscles. You move your muscles when walking and even when you are sitting down. The energy required to work these muscles can come only from one source—food. Your car gets its energy from the petrol—you get yours from your food. On a cold day you notice that you are warmer than the surrounding air—where does your heat come from? From the breaking down and burning up of your food. When your car engine has been running for a time it gets hot, the heat comes from the burning of the petrol to supply motion to the car. Therefore in the act of breaking down foodstuffs to supply energy for movement you are generating a certain amount of heat, and you possess mechanisms in your body to conserve that heat and make yourself more or less independent of the outside temperature. Reptiles such as snakes and lizards do not have this mechanism.

The amount of energy that a food supplies is known as its calorific content. A food that supplies the body with a lot of calories has a high energy value; butter and sugar are examples of foods of this nature.

Some of you may be wondering where your excess weight comes from. The body needs only a certain minimum amount of food to supply its energy. Any extra is stored in the form of fat or perhaps some of it may be excreted through the kidneys. So excess fat can come only from the food. Now certain persons burn up food at a more rapid rate than others, therefore they require more, and often a lean individual eats much more than a fat person for this reason.

In addition to supplying energy, food must, unlike the petrol in the car, replace the tissues as they become worn. Tissues are composed principally of different sorts of proteins and as these are broken down they must be replaced from the food. A slice of steak gives you a good idea of the nature of muscle protein. Proteins themselves are made up of substances called amino acids which might be compared to the bricks of a wall. When a protein such as that contained in meat is eaten, the process of digestion breaks it down into its constituent amino acids. It is just like knocking down the brick wall and separating the bricks from one another. These amino acids are absorbed by the intestines and once inside the body they are reconstructed in the same way that another wall could be constructed from the identical bricks. Some may help to make muscle proteins, some liver proteins,

some blood proteins and so on. Some of them are burnt up as fuel. In other words, besides replacing worn out tissues they supply energy. There are both animal and vegetable proteins.

Some of the energy required by the body comes, therefore, from the proteins. There are two other constituents of the diet which supply energy; these are fats and carbohydrates. Fats are also of two kinds—animal and vegetable. Animal fat is represented by suet, the fat you see in meat, or by butter. Typical vegetable fats are olive oil, cocoa butter, and peanut oil.

Carbohydrates occur mainly in the vegetable world but there are two main sorts—starches and sugars. Many foods contain both, such as the banana and the sweet potato. Cereal grains contain mainly starch, peaches and pineapples mostly sugar. There are many varieties of sugars. Cane sugar is one type; it is called sucrose. Then there is the so-called barley sugar or glucose, fruit sugar or fructose and so on. All these things, proteins, fats and carbohydrates, supply calories and therefore each does its share towards providing energy for the body.

If the production of energy was all that mattered to us, nutrition would be easy. A pound of lard or dripping would give us over 4000 calories and that would more than supply our needs for each day. But we must have our protein, fat and carbohydrate in the proper proportion. Protein we must have to build up our bodies, and we must have a certain amount of carbohydrate to help burn up the fat, otherwise it would be incompletely burnt and a condition called *ketosis* would develop. Then besides these items in our diet we need minerals and vitamins, but we will deal with these in another article.

Now all foods contain one or more of the three elements—protein, fat and carbohydrate. Fish and meat contain two, protein and fat. Potatoes and vegetables contain all three but they have least protein and fat. Nuts contain quite a lot of protein, some fat and carbohydrate.

A word may be said here of the Hay diet. First of all let me tell you that there is scarcely a shred of scientific evidence in its favour and that any good results it is reputed to have achieved have been due probably to psychological effect. There is no evidence that the stomach is unable to deal with proteins and carbohydrates simultaneously; on the other hand, there is some evidence that they aid in the digestion of each other. Since all vegetable foods contain proteins as well as carbohydrates, it is obvious we were not meant to and we cannot eat one without the other, and finally in mother's milk which is acceptable to the tiniest child we find almost equal quantities of protein, carbohydrate and fat. The Hay diet is regarded by all physiologists of any standing as being definitely a fad.

To return to the subject of calories. For the mere working of the body, with a minimum expenditure of energy, when, for example, we are spending a quiet Sunday at home, we need a certain number of calories. On to that amount we must add more calories according to the amount of work we do. The number of calories required for bare existence is given by the League of Nations experts as 2400 per

day. If you do housework you need more calories—the harder you work the more calories you need.

The following list, collected from various authorities, gives an idea of the number of calories required for persons in various occupations:

Teacher or Office clerk	2600 calories per day
Tailor	2750 ,, ,,
Physician	2762 ,, ,,
Housewife, Hand seamstress, or Typist	2800 ,, ,,
Soldier (peace, light work)	3029 ,, ,,
Soldier (war, moderate work)	3146 ,, ,,
Shoemaker	3150 ,, ,,
Carpenter	3194 ,, ,,
Metal worker	3500 ,, ,,
Painter	3600 ,, ,,
Labourers (moderate work)	3611 ,, ,,
University boat crew	4085 ,, ,,
Blacksmith	4117 ,, ,,
Brickmakers	4641 ,, ,,
Stonemason	4850 ,, ,,
Woodcutter	5500 ,, ,,

Women are regarded, generally speaking, as requiring about four-fifths of the number of calories required by men doing the same work.

You see, then, that calories play an important part in our diet; that they are derived from the proteins, fats and carbohydrates in our food and that they supply our energy, replace our tissues as required and if eaten excessively are stored as fat.

If a girl who obtains all the calories she requires from her three daily meals, goes to the pictures or to the theatre and consumes a one-pound box of chocolates, she will be taking in over 2000 extra calories—in other words she will have eaten, in the one day, practically two days' supply of calories. A lot of these will not be utilized, and the unburnt carbohydrate and fat will be stored as fat in the tissues. If this chocolate eating is a regular occurrence, our young woman will find herself with a bulging waistline. She will then cease drinking milk and eating butter in order to keep her weight down. In so doing she will cut out her supply of vitamin A. Her complexion and probably her eyes will suffer and she will become thoroughly miserable—just because she began by eating twice as many calories a day as she needed.

Second Inquest on Detective Stories

By R. Philmore

(In the April number of Discovery *we published an* 'Inquest on Detective Stories', *by R. Philmore and John Yudkin. So many people enjoyed this article that we have asked Mr Philmore for a sequel, this time on the psychology of detective stories. Mr Philmore is an eminent detective story writer himself, and his latest book,* Short List, *is the August choice of the Crime Club.)*

Motives

Most of the famous murders in real life that one remembers have been committed for greed or lust or jealousy. Most of the murders that one does not remember were committed for similar motives, so the statistics tell us. It is perhaps surprising, at first sight, that these motives have not usually been the most successful in the hands of writers of detective stories.

The writers of serious fiction have stuck to these motives when they introduced murder. Some of the best writers have described Dostoievsky's *Brothers Karamazov* as "almost the best detective story ever written", as well as probably the greatest novel. Here the murderer is actuated by greed. So is Raskolnikov in *Crime and Punishment*; and his greed is for a comparatively small amount of money, too. The heroine of *The Idiot* is murdered by a jealous lover.

Disadvantages of Standard Motives

But the use of these motives has certain very definite disadvantages for the detective writer. As a rule greed, lust and jealousy are pretty obvious things, and it is difficult for the writer to conceal his murderer, if that murderer is powerfully actuated by one of them. Either we know the murderer all along, or, when we are told his name, we don't believe it. The serious novelist is concerned to examine the working of a man's mind and the pattern of his motives until they make murder inevitable. The detective writer cannot prove this inevitability without telling us early on in the book who the subject is.

Some good attempts have been made. The great classic, *Mysterious Affair at Styles*, is an example of murder for greed. Here Agatha Christie manages by extraordinarily clever juggling with clues and devices to hide the lack of clear murderous intent in her main villain. She has tried to do the same thing again, quite often. Perhaps her greatest success in this medium was in *Peril at End House*, where she contrived to create a murderer who, we felt at the end, was probably good for anything: but here the motive was more than greed, it had elements of jealousy and revenge. Here, again, any doubts we may have felt during our next-morning reflection as to the psychological state of the murderer were dispelled by the brilliance of the central detective device.

Murder for greed appears in *Have His Carcase*—perhaps the best-worked-out of the books of Dorothy Sayers, and one of the better balanced of them in so far as the powerful influence of Harriet Vane is there to restrain Lord Peter Wimsey, to whom she had not yet surrendered—and in *Hamlet Revenge!*, that splendidly written book by Michael Innes. But in both these cases there is more to it than sheer greed. In the first-named the murderer is concerned to prevent the loss of something he had

counted on; in the second the author almost convinces us that his murderer was ready to commit his crimes for the excitement.

Jealousy and Lust

It is, of course, difficult to make a clear distinction between jealousy and lust as motives for murder, since one is not often present without the other. A number of serious writers have tried to build up a condition in which a character murders for this reason. They have not found it easy. Shakespeare's *Othello* is spoilt for many of us by the unconvincing gullibility of its hero. Moravia's *The Wheel Turns* is a recent splendid failure in this direction. Detective writers have found it even more difficult. Philip McDonald, in *Rope to Spare*, has contrived an atmosphere so intense that we are prepared to believe in his murderer at the end; but our belief scarcely survives reflection. Ellery Queen's masterpiece, *Halfway House*, is so ingeniously contrived, and the final solution so neatly thrust on us, that we overlook the fact that we have not been given much chance to study the murderer and feel his pulse. *The Cask*, which is often erroneously regarded as Freeman Wills Crofts's greatest book, has a convincing enough murderer. But he is so convincing that he is the only possible suspect, and the interest of the reader lies, not in detecting him, but in uncovering the ingenious tricks by which he has laid suspicion on his rival. (Incidentally, it ought to be stated that the charm of Crofts lies very largely in his style: it is so unpretentious and so naturalistic that most of us not only enjoy reading it: we patronize the writer, saying that he does very well without any literary gifts.)

Academic Motives

Severa writers have tried to escape from the difficulty of handling the greed or lust-jealousy motive by inventing somewhat remote motives for their murderers. We have had several murders for altruistic motives. The master of this type is Anthony Berkeley, whose wit and graces are sufficient to make up for shakiness of motive and, sometimes, lack of straight detection. In *Second Shot* he gives us a brilliant essay in this kind of thing. In *Jumping Jenny* he does it again—except that here he adopts the supremely cunning device of pretending to reveal his murderer at the time of the murder. (This is done with one of the most effective colons in literature.) In *Trial and Error* he makes most of us believe that he is doing it again. Ellery Queen has a precious nearly altruistic murder in *Spanish Cape*: here the sheer grandeur of the main device is so powerful that we are too grateful to pick holes in the character of the murderer. Nicholas Blake, in *A Question of Proof*, writes so brilliantly that he almost convinces us that a man could murder for sheer hate; but it would not be wise for a lesser artist to try to copy him.

Revenge

Murder for revenge is usually pretty effective in detective fiction. Here the convincing note is usually provided by making the victim so odious that, as we read, we all want to kill him; so that, when someone in fact does the trick for us, we don't stop to question whether our murderous feelings could in life have been translated into action. Everyone hates a blackmailer; everyone sympathizes with the amiable murderers in Austin Freeman's *Mr Pottermack's Oversight* and Georgette Heyer's *Behold Here's Poison*—two fascinating books. Here, of course, there is the additional motive of stopping the blackmailer from blackmailing. Sheer revenge seems a perfectly adequate motive for the murderer in Nicholas Blake's *The Beast Must Die*, because we all hate motoring murderers. Similarly, the world's horror at kidnapping was capitalized by Agatha Christie in her delightful *Murder on the Orient Express*—a book which, if it is not her best detective story, is certainly Poirot's best and most charming appearance. But, to my mind, the best exploitation of this kind of

motive appears in Carter Dickson's *Plague Court Murders,* which must be put on a list of the dozen best detective stories ever written. Here the motive is revenge, so to speak, for something which has not yet been done—the murder of the murderer. The "atmosphere" is slightly bogus: but it works. Of course, H. M. is so much the best detective that, once having invented him, his creator could get away with almost any plot. In fact he doesn't try to. Incidentally, the method of killing employed in this book is unusual and ingenious: the victim is shot with pellets of rock-salt to give the impression that he was stabbed. I must remember to consult my friend Dr Yudkin as to its feasibility.

Unconvincing Motives

Some writers—a good many of them eminent ones—have been thrown back on what seem to me quite inadequate and unconvincing motives for murder. After all, murder is an unusual activity. We can imagine a professional revolutionary or a neurotic gangster killing without much thought; but when a normal citizen murders in these days it is usually under some terrific compulsion (even though the compulsion may not be evident to the casual observer, and no novelist, not even so humble a craftsman as the detective writer, can be a casual observer). I can't believe in Dorothy Sayers's murderer in *Busman's Honeymoon.* Both the gain and the chances of gain were so small for any but a most abnormal murderer: and this one gives precious little hint of abnormality. (But then, the chief detective interest in this book is not so much in who did the murder as in what Lord Peter and Lady Peter were up to in that bedroom.) Much as I enjoy Philip McDonald's writing, I can't accept most of his alleged motives for murder. I don't believe that his film favourite in *The Crime Conductor* would kill a man who had a slight control over his actions, nor that his eminent statesman in *The Rasp* would kill another eminent statesman because he was slightly more eminent. I have to confess that my admired Agatha Christie sometimes tries to bamboozle me into accepting an unreal motive for murder. I refuse to believe that her famous actor in *Three-Act Tragedy* would commit a murder merely as a rehearsal for another murder (which in itself was scarcely believable). Nicholas Blake's *Thou Shell of Death* is an exciting and brilliantly written affair: but the very skill with which the writer builds up the semi-heroic figure of his hero makes me refuse to consider him as the despicable murderer he is finally revealed to be. I seldom believe in the motives supplied for his murders by S. S. van Dine; I could be induced to believe, for instance, that some human beings might try to run through a whole household, but certainly not the alleged culprit in the *Greene Murder Case.* And, while on the whole I must choose Ellery Queen as my favourite detective writer, I am afraid I raise my eyebrows at some of his motives. I am not convinced that anyone would murder so widely for revenge and petty gain as did his villain in *The Egyptian Cross Mystery*, nor that his unpretentious little criminal in *The Chinese Orange Mystery* would kill a complete stranger for such doubtful gain.

Murder because of the Past

On the whole the safest line to take is to make your murderer afraid of exposure. We can believe that most men, if they have done something really guilty, will kill to hide its coming to light. Perhaps we believe it too easily; it may be because everyone has a shame or a streak of guilt in him, so that threat of revelation strikes a responsive chord. (Murder for gain doesn't do this. How many of us hate the rich profiteers we know strongly enough to dream of killing them?) And there is an advantage for the writer in this kind of motive, since he has two mysteries to hang over us: who did the murder? what was there in his past? Most of the very best

murders in detective fiction have been committed for this reason. I suppose the best judges will without undue anger allow me to mark Dickson Carr's *Arabian Nights' Murder* as the best detective story ever written. Here the motive (though containing elements of revenge) is chiefly to hide the fact that a family has been disgraced by an unfortunate liaison: and we are shown enough of the murderer to believe he had it in him. In *House of the Arrow* and *Murder of Roger Ackroyd* there is the same motive, fear of exposure of blackmailing; and, although neither A. E. W. Mason nor Agatha Christie keeps to the strict known rules, few would regard these two books as other than masterpieces. In *Cards on the Table*, an even better Agatha Christie book, we have a murder committed for fear of exposure of another murder. In that extraordinarily exciting work, *Give Me Death*, Isobel Briggs Myers makes an American commit murder to prevent his negroid ancestry being exposed, and Philip McDonald's best book, *The Noose*, is that in which his murderer has the best motive—fear of being exposed as a coward and a cheat—so that here the combination of McDonald's style and characterization with a really convincing motive makes one of the books we must all put on our favourite shelf.

The Romance of Pearls

By EDWARD SAMUEL

(*A single pearl, valued at ten thousand pounds, has recently been discovered at Broome, Australia. This article tells a few of the tales of fortunes found and lost in the Australian pearling industry.*)

Pearling luggers outside Broome

BROOME, headquarters of the Australian pearling industry, has been thrust into the news after years of depression by the discovery of one of the most wonderful pearls which ever came from the deep. This pearl, which weighs 103 grains, is considered to be the finest ever found in Australian waters. It was found by an Australian-owned lugger off Broome last year; it was valued at £10,000, and has just been brought to Sydney. This wonderful gem recalls the whole history of this romantic industry. In 1699 William Dampier reported the existence of pearl-shell in

Shark Bay, Northern Australia, but it was nearly 150 years later that Lieutenant Helpman, patrolling the North-West coast to prevent the illicit removal of guano, returned with shell actually containing pearls.

For twenty years, the activities of pearling in Australia were shrouded in mystery, and the earliest records of an official nature are dated 1870. In that year shell worth nearly £10,000 was taken and a paltry £50 worth of pearls. The industry progressed, and in 1912 this harvest of the ocean exceeded half a million pounds. Twenty-one years later Western Australia had produced over £10,000 worth of shells and pearls, four-fifths of which was realized from shell.

The beds of Northern Australia are the most extensive in the world, and produce two kinds of oysters—the small Shark Bay, valuable chiefly for pearls (*Meleagrina imbricata*) and the larger shell (*Meleagrina margaritefira*) chiefly valuable for shell.

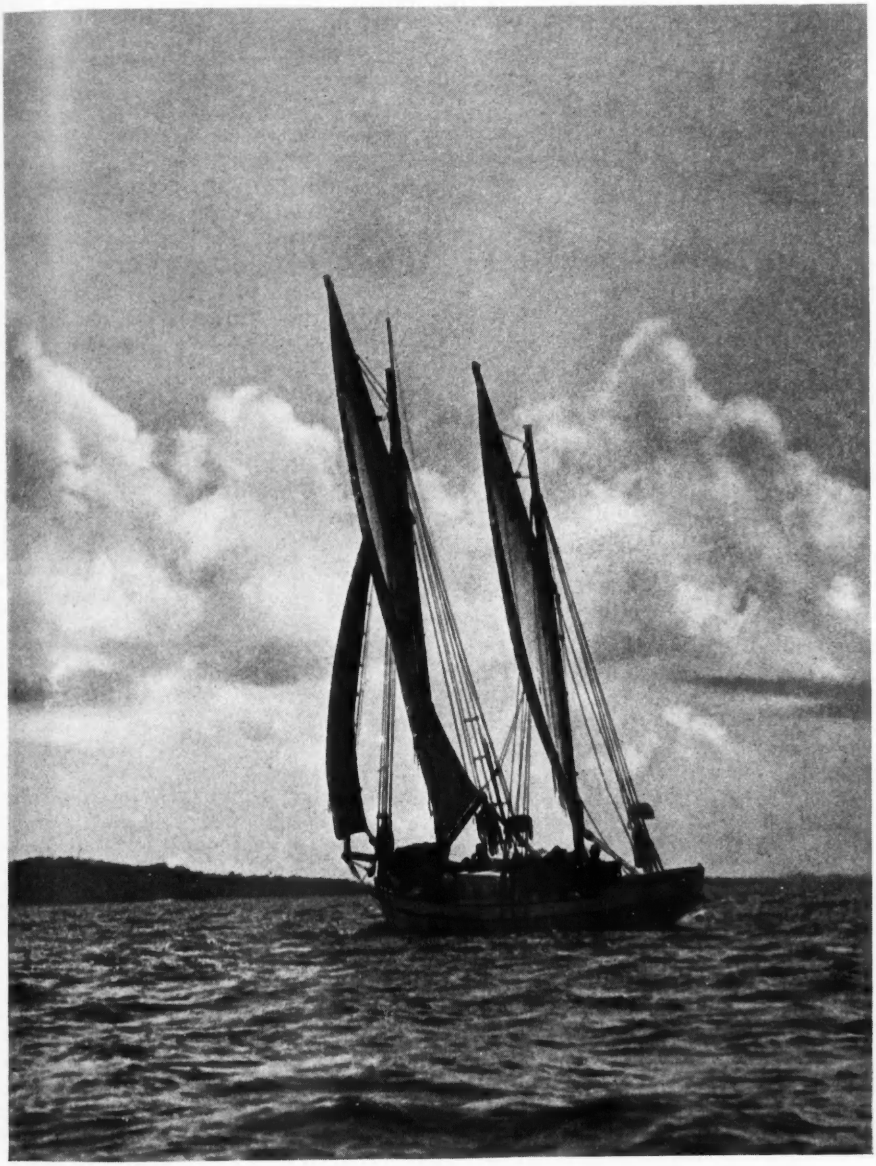
Pearls which are really incidental to the industry are a lottery, mostly blanks, and a few large prizes.

A pearl diver at work on the beds

For years Japanese enterprise has gradually absorbed the Australian industry, and little Broome has slumped badly. It has lost its glamour and history, but this great find may revive it. Trochus and Bêche-de-Mer had as main industries replaced pearling, and where once there were nearly 500 luggers engaged in the trade, there are now a mere fifty. There are no white shell-openers to-day with the fleet, and it is a sorry sight to see deserted luggers high and dry on the beach of Dampier Creek. The town also is suffering from the inevitable decay, and neat home bungalows of better days are at the mercy of white ants, and tenantless. No doubt this great new pearl will be a tonic, and will inspire old hands and new adventurers to seek—what they are most unlikely to find. But the coast to-day re-echoes with stories of great gems of the past which have seen the light of day at Broome. Every year a few Jewish connoisseurs from great Paris houses, for Paris is still the clearing house for pearls, travel across the world and take away a few Broome pearls.

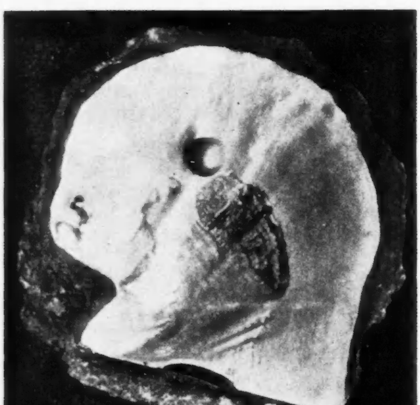
There is indeed romance in the story of great Australian pearls. The first really fine one was Black's in 1873, found where the green turtle abounds, in the Flying Foam passage. Captain Black gave a nigger a broken pocket knife for it and sold it in London for £2500. English jewellers sat up and took notice and looked up Australia on the map.

Tommy Clarke, a well-known figure, died in Broome five years ago, but fifty-five years ago he was dry-shelling on the reefs of Baldwin Creek with an old lubra. He picked up the most wonderful of all pearls, the "Southern Cross". It was 4 in. long, a remarkable jewel of eight pearls joined together in the shape of the Cross. Clarke's father sold it to another character, "Shiner" Kelly, for £10 and a bottle of gin. It proved to be one of the jewel wonders of the world, was sold for £27,000, and exhibited on velvet in the Vatican; it is an historic pearl which will be famous for all time.



A pearling lugger off Broome, Australia

Since the days of "skin" diving many valuable pearls have quietly slipped away from Australia, unheralded and unsung, and certainly unknown to the Customs authorities. All the nations of the world have been lured to Broome by the sheen of pearls and they could tell some thrilling tales, but mostly they have considered discretion the better course, and the clever ruses of "snide" only see the light when plots are discovered. Walters's pearl, as it is known, is an instance of the adventure of pearling. Walters came to Broome on the brig *Lady Dennison* with the fleets from Thursday Island. His Filipino diver found a perfect round pearl and gave it to a Torres Strait woman on board the brig to guard for him until the end of the pearling season. Fate intervened, the *Lady Dennison* foundered in a hurricane with all hands. The Filipino returned and spent months diving at the site of the wreck. The whole fleet was at hand when at last the tale was told. The drowned woman it was known went down with the treasure in a bottle hung round her neck, and was never found. The sea still holds this priceless pearl in its keeping.



The pearl in its shell

One night in 1904 the Jewish buyer Lieblid was murdered on the derelict schooner *Mist*, lying opposite the Roebuck Hotel in Dampier Creek. He had been in negotiation with three desperadoes, a Swede, a Patagonian and a Manilaman, to obtain possession for a paltry £500 of a stone worth £5000, hidden somewhere in the Asiatic quarter, and offered for secret sale. Lieblid is buried in Broome cemetery, and the three guilty men were hanged in Freemantle, but the fatal pearl remains forever a mystery, though it had been seen and certified as 105 grains, and a man had been charged with stealing it.

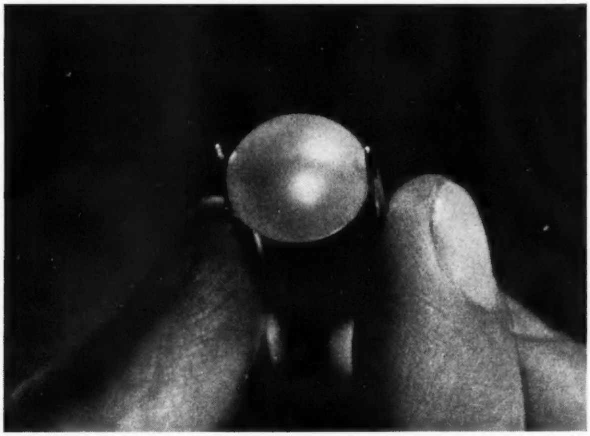
A few years before the War, when the archduchesses of Russia were avid buyers, the coral reefs yielded a glorious single stone that was sold for £8000—we shall call it Duckett's pearl, though that was not the owner's name.

Duckett lived to curse the day he found it. He had worked long in Broome and earned enough to buy a lugger, the *Struggler*. It prospered and he bought another, the *Why Not*. With an honest diver and good results, he christened the next the *Virtue*, and *Virtue* was rewarded, for she brought him the famous pearl. The fourth of his fleet was named in its honour the *Welcome*. He and his wife set forth on a spectacular tour, and returned and bought a cattle station. A shell-opener, who was not on the ship but had a clause in his contract for "20 per cent. commission on all pearls found", cost him £1000 in litigation, and won. The fleet was sold to pay his debts. His wife grew restive, and left him. All he had, to start again with, was enough to build another lugger. He called that the *Experience*. Such is the humour in Broome, where men gamble with pearls and fate in hard times and in hurricanes.

The biggest pearl ever found at Broome was from Moss and Richardson's luggers about twenty-five years ago—a pigeon's egg of roseate light, 160 grains. No pearl-cleaner touched it—it gleamed in perfection when it came from the sea, but the demand was poor at the time, and it realized only £4000. The celebrated "Star of the West", from James Clark's luggers, in charge of Mr J. T. Mackenzie, of Broome, in 1916,

had better luck. A brilliant "drop" of 105 grains, it brought £6500. Not more than three stones have touched the £5000 mark since the War. Seed pearl, flat button, double button, "pear" or "drop", and perfect round—so run the values ascending, and the usual worth of a perfect round is the square of its grain in pounds. A 20 grain pearl of fair quality might be expected to bring £400. But a jewel of distinction is a pearl without price, and the governing factor is particular demand. For a special purpose—a pendant or tiara, or a millionaire's fancy—the right pearl to the pearler is a gift of the gods, as in the present instance.

It is interesting to know that Streeters, the well-known Hatton Garden jewellers, still retain interests at Broome; either they are optimists, or cannot sell. The greatest pearl-cleaner of all time at Broome was a Cingalese—T. B. Ellis. This man could have told many a tale of romance and gorgeous pearls; he probably "dressed" every great stone for half a century and he was a picturesque character. His work was never equalled; as black as the ace of spades, but always smiling, he was discretion personified. His capable ebony black hands, in their time, held all the splendid secrets of the sea, and it would no doubt have grieved him to know that Broome's most wonderful stone should come from the depths of the ocean just too late for his master hands to work upon.



The finest single pearl ever found in Australian waters. Valued at £10,000

Who knows, perhaps this great new pearl, just becoming famous, and as yet unnamed, may imbue a little fresh life into an industry which has, for a decade, been slowly passing away.

Wonders of the Dreaming Mind

II. Telepathy

By R. L. MÉGROZ

(*As before,* Discovery *is glad to publish Mr Mégroz's interesting views, but cannot accept responsibility for them. There are many good reasons why the statistical results claimed by Dr Rhine, if they could be authenticated, would carry greater scientific validity than the individual phenomena. It is worth remarking that responsible opinion does not accept Dr Rhine's results.*)

TELEPATHY, or extra-sensory perception generally (if there is any that could not be described as telepathy), has recently received more scientific attention than has ever been accorded to this alleged faculty of the mind. To admit the truth of a single claim to perception without using the senses would be a revolutionary avowal that the very foundations of our science and philosophy are—to say the least—inadequate. We need not feel surprised therefore if in spite of many recorded experiments which are claimed to have proved thought-transference or some form of "clairvoyance", orthodox science does not yet admit the possibility of this "sixth sense" that the Buddhists recognized long ago as "manaw". But as science is comparatively ignorant still of the nature of mind, the orthodox scepticism need not discourage us from seriously examining the available evidence. It is worth remembering that many of the most intelligent and creative minds in the history of civilization have been unscientific by our modern standards, and the person who already has a scientific reputation is perhaps naturally loath to confess a belief in evidence that will not satisfy other scientists. Camille Flammarion, eminent French astronomer, and also eminent for his researches into mysterious psychic phenomena, took the risk, and in his Introduction to *The Unknown* he made a complaint that would still apply, though possibly in a lesser degree so far as public opinion generally is concerned.

"It must be owned", he said, "that work of this kind is interesting—passionately interesting to the writer while searching for truths unacknowledged or unknown, but it is, from the point of view of public opinion, labour without reward. Everybody, or almost everybody, has a poor experience of those who undertake it. Men of science think it is not a scientific subject, and that it is a pity to waste time over it."

Yet Flammarion did not come to the study of "supernormal" psychic phenomena in old age, after doing all his really scientific work and degenerating—according to sceptics—into a pathetic credulity. No; he was occupied by the study all his life, while also studying astronomy. At least in the sphere of telepathy, the abundant records supplied by him have been subsequently justified by other (and often more carefully made) records, and it is possible that a fresh kind of evidence in support of extra-sensory perception has been accumulated by the intensified study of dreams since he wrote at the end of last century. The reader of *The Unknown* will remember the frontispiece showed a series of drawings of objects in pairs, one of each pair being described as the original, the other being the approximate reproduction of the image by the person who received the impression mentally from another's mind. This kind of test—which we may call voluntary—has often been tried in various forms during the past half century. Many results were recorded in F. W. H. Myers's *Human Personality* (vol. I), and by the Society for Psychical Research. The American writer, Mr Upton Sinclair, has recently published results of experiments with his

wife which are amazing if they can be taken at their face value. But whereas he recommends concentrating by the method of emptying the mind of ideas before receiving the mental impression, other experimenters have discovered that thought-transference occurred without voluntary control of the mind and with better results. This point becomes important in a case like that of the authentically recorded experiments of Miss H. Ramsden and Miss C. Miles, which Prof. W. F. Barrett, F.R.S., described in his book, *Psychical Research* (Home University Library, Butterworth). Miss Miles, in London, was the "agent", sending the thought messages to Miss Ramsden, twenty miles away, at Gerrard's Cross, Buckinghamshire. While testifying that he had examined the documents, and found conclusive evidence of the transmission of telepathic impressions at a distance, Prof. Barrett observed that "the best results appeared to be obtained when there was *no special effort* made by the transmitter—confirming our previous inference, that it is the subconscious intelligence, which is operative in these and other supernormal psychical phenomena. In fact, Miss Miles writes that she found it was much easier to impress an idea without specially concentrating her mind on it at a fixed time."

The experience of Miss Miles and Miss Ramsden is probably very common, for although it is difficult to assess the value of the testimony, many people can tell of having received or transmitted thoughts to another person. Presumably a favourable condition for such thought transference arises from the "tuning-in" of minds to each other by sympathetic contacts; certainly one most often hears or reads of telepathy between close friends or husbands and wives. The main difficulty in such cases is to determine the part played by suggestion, which may occur without being noticed. Thus if I and a friend suddenly make a remark together on the same subject, although the subject may apparently be quite disconnected from what we have previously been talking about, an infinite variety of associations may have led our minds to it along the same route.

The possibilities of self-deception are so numerous, that psychologists much prefer the routine experiments, as with cards, that can be repeated at will. Such experiments, however, set up other possibilities of error, notably when results are calculated by the statistical method employed by Dr Rhine, of Duke University, California. In his *New Frontiers of the Mind* he has described the tests made with special sets of Zener cards, claiming results that prove the possibility of extra-sensory perception. The enormous repetition of the tests is claimed to make statistical calculations more reliable, so that if a certain average of correct answers is reached, the possibility of chance in guessing what is on the cards drawn from the pack by the dealer can be eliminated mathematically.

Now the results recorded by Dr Rhine are quite conclusive as to the existence of extra-sensory impressions, covering tests made at a distance as well as in sight of the cards, but they have already been discredited in the eyes of most psychologists, receiving severe criticism in America as well as in Britain. The most damaging discovery was that owing to the careless use of the same packs of cards in thousands of tests, it became possible to detect small marks and differences on the backs which enabled anyone to guess the symbol on the other side. Mr S. G. Soal, Senior Lecturer in Pure Mathematics, Queen Mary College, University of London, has stated that he and colleagues in the Psychology Department of University College, London, conducted over 100,000 of the Rhine tests "without finding a single individual who was able to demonstrate these alleged powers", and Prof. Thouless of Glasgow has announced the same negative result after many thousands of similar tests. Mr Soal added that "last summer Mrs Eileen Garrett, one of the subjects with whom Dr Rhine claimed to have obtained results of an astronomical order, was tested with over 12,000 guesses in the psychological laboratory at University College by myself and

eleven other persons. The results were completely negative."

Where are we, then, after all this industry? Prof. Thouless, in answer to a question of mine, wrote: "I do not claim that my negative results disprove Dr Rhine's positive ones. I started to repeat Dr Rhine's experiments in the hope that I should confirm his results. I have completely failed to do so.... I do not think that extra-sensory perception (if it exists) is satisfactorily explained until we know why it occurs in North Carolina but not in London or Glasgow."

This reduction of the whole matter to absurdity is really uncalled for. If Prof. Thouless believes that the Rhine tests are useful (and if he does not, why repeat them thousands of times?), then he must have the courage of his laboratory and say that he has disproved Dr Rhine's results, for his conclusion that extra-sensory perception may be possible in North Carolina but not here cannot be taken seriously. Moreover, he has on his (and Mr Soal's) side the serious flaw in the Rhine evidence which has been referred to. He, however, does admit that the fault of the cards that could be recognized from the backs does not affect all the Rhine tests, some of which were conducted at a distance, and others by the subject naming the cards without having them removed from the pack.

The most serious objection to these routine tests, enormously repeated, is the difficulty of really controlling the essential conditions which may be supposed to make so subtle a manifestation of thought obvious. It is agreed that some people may be more sensitive percipients than others, but for the purpose of such tests they are selected almost at random—all sorts of people take part, and in incalculable states of mind. As Mr Kenneth Richmond wisely remarked when reviewing Dr Rhine's book: "We have no assurance that the intuitional faculty, if there is one lurking in the unconscious mind, is necessarily interested in scoring on the target that the experimenters set up; it may in given circumstances decline to provide evidence, and mix right and wrong answers in an irresponsible pattern."

Hence, surely, the greater value of the kind of evidence already referred to, as was described by Prof. Barrett, which is completely ignored by Prof. Thouless's farcical summing-up. Hence also the value of many curious records of dreams that seem to imply telepathy or some other form of extra-sensory perception. Yet Prof. Thouless, again responding to a question of mine, described Dr Rhine's experiments as "of much more value than evidence from dreams, since their results can be estimated statistically". So far, on his own showing, the statistical experiments of the Rhine type have proved nothing except that the mysterious faculty of mind of non-sensory perception exists only in Carolina. But support for him exists in a quarter that he may not welcome. At the time of the foregoing controversy, a correspondent, Miss Isabel Kingsley, who has studied much "occult" literature, reminded me that Mrs Besant in *The Ancient Wisdom* stated some thirty years ago that extra-sensory perception was an evolving faculty of mankind which would be first observed in the United States of America, notably along the Pacific sea-board. A minority of young people there, she declared, already possessed this faculty. The idea behind this assertion was a belief that the more highly electrically charged atmosphere had this effect upon the population. This theosophical speculation is interesting, but not even Prof. Thouless will say that it is more than a speculation.

Some kind of clairvoyance was among the earliest of the phenomena noticed as occurring in the condition of hypersensitiveness resulting from hypnosis, and a similar mysterious faculty was observed in sleep-walkers. The hypnotic subject is really a sleep-walker. The earlier records, however, were not carefully made and lack authenticity. At the same time that Flammarion was collecting evidence of all kinds of mysterious alleged phenomena, medical men were practising and studying hypnotism, and during the last quarter of the nineteenth century a great deal of properly

recorded evidence for some form of extra-sensory perception in hypnotized patients was collected. A typical case will suffice for illustration. It was supplied by C. Richet to the S.P.R. and is quoted by Barrett:

"On Monday, 2 July 1888, after having passed all the day in my laboratory, I hypnotized Léonie at 8 p.m., and while she tried to make out a diagram concealed in an envelope I said to her quite suddenly: 'What has happened to M. Langlois?' Léonie knows M. Langlois from having seen him two or three times some time ago in my physiological laboratory, where he acts as my assistant. 'He has burnt himself', Léonie replied. 'Good,' I said, 'and where has he burnt himself?' 'On the left hand. It is not fire: it is—I don't know its name. Why does he not take care when he pours out?' 'Of what colour', I asked, 'is the stuff which he pours out?' 'It is not red, it is brown; he has hurt himself very much—the skin puffed up directly.'

"Now, this description is admirably exact. At 4 p.m. that day M. Langlois had wished to pour some bromine into a bottle. He had done this clumsily, so that some of the bromine flowed on to his left hand, which held the funnel, and at once burnt him severely. Although he at once put his hand into water, wherever the bromine had touched it a blister was formed in a few seconds—a blister which one could not better describe than by saying, 'the skin is puffed up'. I need not say that Léonie had not left my house nor seen any one from my laboratory. Of this I am absolutely certain, and I am certain that I had not mentioned the incident of the burn to any one. Moreover, this was the first time for nearly a year that M. Langlois had handled bromine, and when Léonie saw him six months before at the laboratory he was engaged in experiments of quite another kind."

In the present state of our ignorance it is not possible to say whether such a case indicates telepathy (the knowledge passing from the mind of the hypnotist to the subject) or some form of what is dangerously termed clairvoyance, by which the subject's mind seems to travel away from the body. One may reasonably prefer the hypothesis of telepathy here, though, as Barrett observed, many other records of hyperaesthesia in hypnosis show some form of clairvoyant perception without leaving room for telepathy, at least so far as our knowledge goes.

Before surveying the evidence of dreams, I will only remark that the phenomena of hypnotism are just as mysterious as the most astonishing claims ever made for experimental telepathy, and it is up to our sceptical psychologists to state why they cannot accept the evidence already accumulated by scientific men for extra-sensory perception, instead of relying on ill-conceived, endlessly repeated, routine tests in card-reading.

NEXT MONTH: DREAM-TELEPATHY

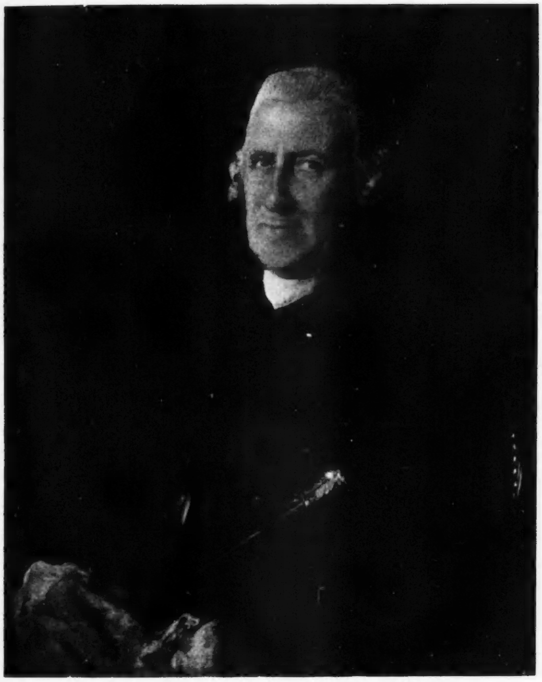
The Fitzwilliam Museum at Cambridge

By Giles Robertson

WHAT strikes the visitor, perhaps, most, in the Fitzwilliam Museum is the diversity of the collections, and the unity of the Museum as a whole. Undeniably this unity is chiefly due to the skill with which the late Director, Sir Sydney Cockerell, arranged the collections, but it may also be traced back to the fact that their nucleus was the collection of Richard, seventh Viscount Fitzwilliam of Merrion, the Founder, which, like all personal collections, was impressed with the unity of the collector's personality. It is from this collection, bequeathed to the University in 1816, that the present Museum has grown, and although the scope of the Museum has been widened by the generosity of later benefactors, it still keeps the character of a Fine Art collection which marked the Founder's bequest. Those sections of the bequest which cannot be called Fine Art, the printed books and printed and manuscript music, are less well known to the general public, though the latter collection is familiar to students of old music throughout the world and is certainly worthy of accommodation in a separate Music Room, like that already set aside for illuminated manuscripts, and which, it is hoped, will eventually be provided.

In addition to the various collections, the Founder bequeathed £100,000 to the University, in the New South Sea Annuities, out of which he directed that "a good, substantial, convenient Museum Repository or other building" should be erected. The older portion of the present building was the result of this decision, but it was not even begun until 1837, more than twenty years after the Founder's death, the interval being spent in an interminable discussion of possible sites. It was designed by George Basevi, a pupil of Sir John Soane and the architect of Belgrave Square, who superintended the work until his tragic death at Ely in 1845, by which year the building was sufficiently complete to be used for the meetings of the British Association. Until 1848 the work was continued by C. R. Cockerell, R.A., but in that year it was held up for want of funds, and the collections were installed and arranged. The building was then complete except for the decoration of the entrance hall and of the

lower North Gallery, now the Egyptian Room. It is known that Alfred Stevens the sculptor worked under Cockerell on the plaster work in the building, but it does not seem that any of the surviving work is from his hand. He probably worked on the decorations of the Entrance Hall, which were destroyed when the younger Barry



By Gainsborough: Hon. W. Fitzwilliam

completed its decoration in 1875, largely remodelling it. All that we see there at present is Barry's work.

Naturally enough, with the increase of the collections, this building did not remain adequate for the needs of the Museum, but no additions could be made for many years, owing to shortage of money, although a certain relief was obtained by the removal of the teaching collection of casts to a separate building in 1883. But with the bequest of Charles Brinsley Marlay in 1912, in importance second only to that of the Founder, a substantial expansion became possible, for in addition to his artistic collections, he left property and investments totalling £90,000. Out of this sum a site was secured and plans were drawn up by the firm of Smith and Brewer, for a very considerable extension. It was proposed ultimately to build a hollow square of buildings, connected with the old Museum by a corridor gallery in two stories, and it was hoped to proceed with this corridor gallery and the near side of the quadrangle immediately. The outbreak of war, however, necessitated the postponement of all work, and in 1920, when the scheme was taken up again, the greatly increased cost of building only allowed of the erection of the corridor gallery together with a Coin Room and Manuscript Room, paid for by Captain N. M. McClean.

The Marlay Galleries were opened in 1924 and, owing to the generosity of three members of the Courtauld family, the scheme was soon continued and the first side of the quadrangle was opened to the public in 1928. The Henderson bequest made a further addition possible, being the beginning of the street side of the projected court, containing the Henderson armour gallery, the Medieval Room, the Print Room, the gift of the Honorary Keeper, Mr John Charrington, and a new picture gallery.

The collection of paintings, exhibited for the most part in the upper galleries, is probably the best known part of the Museum. Among the paintings many important works came with the Founder's bequest: Titian's "Venus and the Lute Player" (see page 311), the subject recently of an interesting discovery in artistic research, Veronese's "Hermes, Herse and Aglauros", Palma Vecchio's "Venus" and Rembrandt's "Por-

trait of an officer", now recognized as a self-portrait, and a number of Dutch Cabinet works including the three Jan Steens. The majority of the Dutch Cabinet works came, however, in the bequest of Daniel Mesman in 1834, the first substantial acquisition after the Founder's bequest. A collection of Rubens's sketches was bequeathed by the Rev. R. E. Kerrich in 1873.

The basis of the collection of early Italian pictures was laid by the exceedingly judicious purchase, in 1893, of fourteen works from the collection of Charles Butler, including the grand Pisan Crucifixion of the thirteenth century, the Triptych by Simone Martini and the lovely little Madonna by Lorenzo Monaco. Some excellent fifteenth-century works were added in the Marlay bequest in 1912, which also included a number of early Flemish paintings. Further important contributions came from the Fuller bequest of 1923, including the two exquisite predella panels, the "Annunciation" and the "Miracle of Saint Zenobius" by one of the rarest and loveliest of Florentine masters of the fifteenth century, Domenico Veneziano.

The Museum also possesses a few fine Flemish and Spanish pictures and a good collection of English works; including portraits by Hogarth, Reynolds and Gainsborough, including an enchanting early work "Heneage Lloyd and his Sister", and landscapes by Wilson and Constable as well as a number of works by William Blake and characteristic paintings by most members of the Preraphaelite brotherhood. There is also a fine collection of English watercolours, exhibited in the gallery in Room 111, containing works by Cozens, Girtin, Turner, Constable, Cotman and De Wint.

Curiously enough, the Founder's bequest contained virtually no Old Master drawings and though a few fine drawings found their way into the Museum from time to time, as those included in the Kerrich bequest of 1873, the basis of a systematic collection was only laid in 1913 through the gift of Mr Clough of his small but fine collection, which contains one work each by Raphael and Michaelangelo and part of a Botticelli cartoon. The value and range of the collection was immeasurably increased last year by the bequest by Charles Shannon of the collection formed by Charles Ricketts and himself, which includes superb works by Rembrandt, Titian and Watteau. This collection, though still small, also thoroughly deserves accommodation in a separate room.

The Founder's collection of prints, on the other hand, was of the very first quality, being especially rich in the work of Rembrandt and of the early German engravers, and this collection has been steadily increased by gift and purchase, especially during the last thirty years, by the generosity of the present Honorary Keeper, who has also provided fitting accommodation for the collection in a separate Print Room with a Students' Room and Exhibition Gallery.

The Ceramic collection is one of the most important and popular in the Museum, and one whose prominence is recent, dating from 1928 when Dr Glaisher left it his collection of early English and other pottery, together with money for its exhibition and upkeep. The Museum already possessed a fine collection of Italian Majorica, bequeathed by the Rt Hon. F. Leverton Harris in 1922, and a small collection of oriental ceramics has been formed, as well as a collection of porcelain, displayed, perhaps not yet to the best advantage, on the upper landing of the front hall.

A small but fine collection of medieval antiquities is to be seen in a small room off the Henderson armour gallery. It contains a number of enamels and some magnificent ivories, including the celebrated Carolingian plaque of a Pope or Bishop blessing, to which the companion leaf is at Frankfurt. These, with other ivories and enamels, were included in the McClean bequest.

Older and more important are the classical and Egyptian collections, but they are probably less generally appreciated, owing partly to the imperfect lighting of the gal-

leries, and partly to inherent difficulties in displaying the material attractively, especially in the case of the Greek vases. The motive in forming the classical collections was principally the desire to provide a teaching collection for Archaeological students in the University, and with this object in view Col. Leake's collection was bought in 1863. Regular purchases and gifts have greatly strengthened this collection, and important additions came in the Ricketts and Shannon collections. The Egyptian collection is small, but contains some fine sculpture, especially notable is the granite head probably of Sesostris III, the gift of Mr F. W. Green. The important collection of coins is naturally better known to students than to the general public. Its nucleus was the Leake collection, but the importance was greatly increased between 1906 and 1912 during which years Mr J. R. McClean presented the collection begun by his father Frank McClean and enlarged by himself. Recently a number of Cambridge Colleges have generously placed collections on permanent loan.

Lastly we may mention the Library. The Founder's collection again forms the most important section which comprises a well-stocked scholar's Library of the period, in addition to a collection of manuscripts mentioned above and early printed books. These have not been systematically added to, as the chief repository for early printed books in the University is, naturally, the University Library, but the McClean and Marlay bequests contained many fine volumes, which form, with the Founder's books, an important series. But the principal effort in the Library has been to form an efficient Art-Reference Library, and there is now a collection which, though by no means exhaustive, is of great value to students, and is open to any member of the public wishing to study there.

TITIAN: Venus and the Lute Player

In our October issue we shall include a survey of this year's meeting of the British Association, under the title of "Science for the Modern World". The Association is devoting much of its time to science in relation to (1) Warfare, (2) The structure of Society, (3) Education, (4) The particular kind of thought which should be the basis of Education. We shall take these topics as texts and develop them for the benefit of those of our readers who were unable to hear the discussions themselves. Meanwhile we are able to include the actual statement of the President of the Association, Lord Rayleigh, on Science and Warfare. Closely related in tone was the speech made by Professor R. V. Southwell, F.R.S., President of the Engineering Section, and we hope to be allowed to print his address also in our next issue.

Notes of the Month

Science and Warfare

LORD RAYLEIGH: During the Great War itself, few scientific men in any country doubted that it was their duty to do what they could to apply their specialized knowledge to the purposes of war; nor was it often suggested by publicists that there was any countervailing consideration: on the contrary they urged strongly that our resources in this direction should be efficiently mobilized. It is chiefly in vague general discussions that the opposite view becomes vocal.

Science, it is urged, is the source of all the trouble: and we may look to scientific men for some constructive contribution to finding a remedy. It is worth while to inquire what basis there is for this indictment, and whether, in fact, it is feasible for men of science to desist from labours which may have a disastrous outcome, or at any rate to help in guiding other men to use and not to abuse the fruits of those labours. I may say at the outset that I have no sanguine contribution to make. I believe that the whole idea that scientific men are specially responsible is a delusion born of imperfect knowledge of the real course of the process of discovery. Indeed, very much the same complaint was made before the scientific era. Let me refer you to Shakespeare's play of *Henry IV*:

> Great pity, so it was
> This villainous saltpetre should be digged
> Out of the bowels of the harmless earth
> Which many a good tall fellow had destroyed
> So cowardly.

The quotation leads us to inquire how far the further development of this particular kind of frightfulness into modern high explosives was deliberate or not.

In the course of systematic study of the chemistry of carbon compounds it was inevitable that the action of nitric acid on substances like benzene, toluene, glycerine, cellulose and the like should be tried. No one could foresee the result. In the case of benzene, we have nitrobenzene, the key to the aniline dye industry. In the case of glycerine, Sobrero obtained in 1846 the highly explosive liquid called nitro-glycerine. He meant no harm, and in fact his discovery lay dormant for many years, until Nobel turned his attention to the matter in 1863, and showed how, by mixing nitro-glycerine with other substances, solid explosives could be made which admitted of safe handling. Dynamite was one of them. They proved invaluable in the arts of peace, e.g. in mining and in making railway tunnels, such as those through the Alps. They were used by the Irish Fenians in the dynamite outrages of the 'eighties. These attempted outrages were not very successful, and so far as I know no one was inclined to blame science for them, any more than for the Gunpowder Plot. Like the latter, they came to be considered slightly comic. If anyone doubts this, he may agreeably resolve his doubts by reading R. L. Stevenson's story *The Dynamiter*. At all events, high explosives had been too long in use in peaceful industry for their misuse to be laid directly to the account of science.

Coming next to poison gas. We read that Pliny was overwhelmed and killed by sulphur dioxide in the eruption of Vesuvius in A.D. 79. During the Crimean War, the veteran admiral Lord Dundonald urged that the fumes of burning sulphur should be deliberately used in this way, but the suggestion was not adopted. Even if it had been, scientific research *ad hoc* would obviously have had little to do with the matter. During the Great War, chlorine was used on a large scale. I need hardly insist that chlorine was not isolated by chemists for this purpose. It was discovered 140 years before, as a step in the inquiry into the nature of common salt.

Coming to the more recondite substances, we may take mustard gas—really a liquid—as typical. It is much more plausible to suggest that here was a scientific devilment, deliberately contrived to cripple and destroy. But what are the real facts?

Referring to Watts's *Dictionary of Chemistry* (edition of 1894), there is an article of less than forty words about mustard gas (under the heading of dichlordiethyl sulphide). After the method of preparation used by Victor Meyer has been mentioned, the substance is dismissed with the words, "oil, very poisonous and violently inflames the skin. Difference from diethyl sulphide".

There are sixteen other compounds described at comparable length on the same page. So far as I know, none of them is of any importance. A not uncommon type of critic would probably say that the investigation of them had been useless, the work of unpractical dreamers, who might have been better employed. One of these substances, namely mustard gas, is quite unexpectedly applied to war, and the production of it is held by the critics to be the work not of dreamers, but of fiends whose activities ought to be suppressed! Finally at the bottom of the page begins a long article on chloroform. This substance, as you know, has relieved a

great deal of pain, and on the same principle the investigator who produced it was no doubt an angel of mercy. The trouble is that all the investigators proceeded in exactly the same spirit, the spirit that is of scientific curiosity, and with no possibility of telling whether the issue of their work would prove them to be fiends, or dreamers, or angels.

Again, there is the terror of thermite incendiary bombs, spreading fire broadcast through our great cities. The notion is sometimes encountered that thermite was invented for this purpose. Nothing could be further from the truth. I first made acquaintance with it myself in 1901 by hearing a lecture at the Royal Institution by the late Sir William Roberts Austen on "Metals as Fuel".* He drew attention to the great amount of energy which was liberated when aluminium combined with oxygen, and showed how aluminium powder mixed with red oxide of iron would react violently with it, withdrawing the oxygen from the iron, and becoming brilliantly incandescent in the process. He showed further how this mixture, called thermite, could be used for heating metal work locally, so as to make welds, e.g. in joining two iron pipes end to end. I venture to say that it never occurred to him or to any of his hearers that thermite had any application in war.

In discussions of this kind a distinction is often implied between what I may call old-fashioned knowledge and modern scientific knowledge. The latter is considered to be the special handmaid of "frightfulness". The futility of this distinction is easily seen by considering a special case. Iron is thought of as belonging to the pre-scientific era, while aluminium is thought to belong to the scientific era. From the standpoint of chemistry both are metals, and the problem of producing them in either case is a chemical one. When produced they both have their function in "frightfulness": iron to cut and stab; aluminium to make thermite bombs to burn and destroy. If modern science makes its contribution to "frightfulness" in giving us aluminium, ancient craft did so in giving us iron. It is obviously absurd to make any distinction in principle between the two cases. Science properly understood includes all real knowledge about material things, whether that knowledge is old or new.

All these terrors have only become applicable against a civilian population by the development of aircraft. Military objects were certainly not the incentive of the successful pioneers of artificial flight. They were fascinated at first by the sport of gliding, and afterwards by a mechanical transport problem.

It is true that brilliant writers of imaginative fiction, such as Jules Verne and H. G. Wells, had foretold all, and more than all, the horrors that have since come to pass. But it is perhaps more to the point to inquire what were the contemporary views of practical men. The Wrights made their first successful flight in 1903. In 1904 I myself heard the then First Sea Lord of the Admiralty repudiate with scorn the suggestion that the Government were interesting themselves in the matter; and I know with equal definiteness that even as late as 1908 the chief of the Imperial

* *Proc. R.I.*, 23 Feb. 1901, vol. XVI, p. 496.

General Staff did not believe in the military importance of flight. Would it be fair then to blame the inventors for not having realized it, and for not having stayed their hands?

Summing up what may be learnt from the experience of the past, I think we may say that the application of fundamental discoveries in science to purposes of war is altogether too remote for it to be possible to control such discoveries at the source.

For good or ill, the urge to explore the unknown is deep in the nature of some of us, and it will not be deterred by possible contingent results, which may not be, and generally are not, fully apparent till long after the death of the explorer. The world is ready to accept the gifts of science, and to use them for its own purposes. It is difficult to see any sign that it is ready to accept the advice of scientific men as to what those uses should be.

Can we then do nothing? Frankly, I doubt whether we can do much, but there is one thing that may be attempted. The Association has under consideration a division for study of the social relations of science which will attempt to bring the steady light of scientific truth to bear on vexed questions. We rejoice to know that our distinguished American visitors are in sympathy with this aim, and we hope that our discussions with them will bear useful if modest fruit in promoting international amity.

The Age of Indian Mountains Determined in Vienna

The age of the *Deccan Traps* has been a much-discussed problem during these last few years. These enormous masses of basaltic lava flows extend over 250,000 square miles from Bombay all through the Indian peninsula in an average thickness of several thousand feet. Their age is hard to determine because they are in the main of terrestrial origin, whereas our geologic chronology has been primarily based upon the succession and fossil contents of rocks that have been formed in the sea. Only in one place, at Rajahmundry near the Bay of Bengal, are marine beds intercalated between the terrestrial strata, thus giving a clue into which of the known geological formations the latter should be ranged.

The theory that the Deccan Traps are of Cretaceous origin has been shaken recently by two young native geologists, of the University of Mysore, S. R. Narayana Rao and K. Sripada Rao, who found between the lava layers remainders of land plants and fishes and fresh-water algae which seemed to belong to the Old Tertiary period. Moreover in the Rajahmundry limestone they discovered marine shells not yet studied in this light and also specimens of a family of marine algae about which a great many papers have been prepared in the Vienna Museum of Natural History. These are known, by Prof. Julius of Pia, as "whorl algae". They sent their material to Vienna, Professor Pia examined it and ascertained that it contained no forms exclusively known from the Cretaceous, but several characteristic for the Old Tertiary period. *G. Rabel, Vienna.*

A Plea for Quails in England

In this country quails have had less than a fair deal from the more "civilized" section of the community. Netted on the Continent with the assistance of decoys which are blinded with red-hot irons, imported alive, fattened in vast numbers in mews (over 30,000 were burnt to death when there was a fire at some Wood Green aviaries thirty years ago), these gentlest of wild birds have been commonly eaten in their natural nesting season by gourmets. Happily, however, last year was "a quail year", not only in the sense that many specimens were seen and heard in widely different parts of England, but also because the Quail Bill was passed, and the importation of live quails into the United Kingdom between 14th February and 1st July was thereby made illegal. It is possible that this bill, which is only one of various protective measures others having been passed in Egypt, Italy and France, may in time help the species to recover its former status, so that we may again count quails among our familiar summer residents.

A century or two ago quails were particularly plentiful on the Kentish bank of the Thames estuary, and the accompanying photograph of a quail's nest was taken —as late as August—in a barley field no great distance from Canterbury. The species has also been reported from Suffolk, Norfolk, Wiltshire and North Devon during the last two years. It is most earnestly to be hoped that anyone finding quails' nests will do all they can for them. Sometimes it can be arranged that a nest in a hay or corn field shall not be "cut out" at haysel or harvest: a little patch of grass or corn may be left to protect it. Or, if a nest is not seen until the mower has uncovered it, an enthusiast might even be able to arrange for the transference of the eggs to a bantam's care—as gamekeepers do with partridges.

In conclusion, it may be noted that our quails probably do not, as has sometimes been suggested, come from Egypt. It is more likely that they come from West Africa, via Morocco and Spain. Recovered ringed specimens suggest that the Egyptian quails go to Russia, and those which pass through Italy probably nest in Central Europe.

J. D. U. Ward.

ivilized"
of decoys
mbers in
od Green
nly eaten
by gour-
year was
ense that
heard in
, but also
ssed, and
into the
February
e illegal.
which is
measures
n Egypt,
help the
status, so
s among

bank of
vas taken
ury. The
h Devon
e finding
hat a nest
patch of
ower has
erence of

as some-
me from
gest that
ably nest
U. Ward.